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Contents

Geomorphology as a Tool for the Interpretation of Geology and Earth History. *By* JOHN L. RICH 188
Mesons and Nuclear Forces. *By* HIDEKI YUKAWA 192
Symposium on Individual Blood Differences:
Introduction. *By* E. D. GOLDSMITH 198
The Rh-Hr Blood Types: Serology, Genetics, and Nomenclature. *By* A. S. WIENER 199
A Brief Review of the Newer Blood Factors. *By* PHILIP LEVINE 205
Erythrocyte-Isoantibody Reactions in Dogs. *By* L. E. YOUNG, W. A. O'BRIEN, G. MILLER, S. N. SWISHER, D. M. ERVIN, R. M. CHRISTIAN, AND C. L. YUILE 209
The Identification of Hemagglutinins with Red Cells Altered with Trypsin. *By* R. E. ROSENFIELD AND P. VOGEL 213
Treatment of *Erythroblastosis fetalis* by Exchange Transfusion. *By* G. J. BRANCATO 220
The Effect of Speed and Size on the Performance of a Model Ship Propeller at Optimum Efficiency. *By* B. V. KORVIN-KROUKOVSKY 225
The Adjustment of the Blind. *By* HECTOR CHEVIGNY 233
Homothallism and Heterothallism in *Glomerella*. *By* L. S. OLIVE 238
The Truffle Fungi. *By* DONALD P. ROGERS 242
The Chinese of Hawaii: Their Role in American Culture. *By* F. L. K. HSU 243
Problems in Airline Meteorology. *By* P. E. KRAGHT 251
New Members 255

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GEOMORPHOLOGY AS A TOOL FOR THE INTERPRETATION OF GEOLOGY AND EARTH HISTORY*

By JOHN LYON RICH†

Since the introduction of the airplane and of stereoscopic aerial photography, the importance of geomorphology as a branch of geological science has increased greatly. Geomorphology has always been an important foundation subject for geography, but for geology it has often been merely that minor adjunct which, along with "Geographical Location," had its customary place at the beginning of a geological report. Aerial photography and the possibility of aerial reconnaissance have made it a leading and essential tool for geological interpretation and mapping. The geomorphologist may now obtain commanding and revealing views of the landscape from the most advantageous points. Through vertical and oblique stereoscopic photography, he can now examine and re-examine at leisure all details of topography. He can choose his own most advantageous viewpoints and thus gain an overall picture of the region, free from the deceptive effects of foreshortening that make "skyline" geomorphology from the ground undependable. A geomorphologist who has never experimented with stereoscopic oblique, as well as vertical, photography can scarcely realize the tremendous aid which it can bring to his studies.

When geomorphology has developed further as a science along some of the lines to be pointed out here, most of the geologic features of a region, capable of topographic expression, can be interpreted from the air, even through a cover of vegetation. Any doubtful interpretations can be checked later on the ground. But the geologist going into a region carrying a geologic map representing the interpretation of the aerial photographs, together with the original photographs on which all stations located by photogrammetric intersection have been marked by circled dots, will be able to go directly to the critical points and will have before him all the features which show clearly from the air, many of which he might never find by ground work alone.

Geomorphology in its Infancy. Geomorphology now seems to have reached the end of one era and the beginning of another. At the moment, geomorphologists are groping to find the best road along which to advance. The brilliant work of Gilbert on stream sculpture and the work of Davis on the cycle of erosion opened a way which numerous enthusiasts followed, carrying the ideas of the cycle and of peneplanation to such an extreme as to stifle the development of other lines of investigation. Thus, for more than a generation, we have had a paucity of new ideas and comparatively little advancement of the science. During that time, Walther Penck challenged Davis's idea, and, though many aspects of his work are open to question, his emphasis on interstream degradation or mass wasting stands as a highly important contribution.

As to the future, some seem to feel that quantitative studies and the statistical approach may be the best line for further progress. The writer believes, however, that the enormous complex of variables governing the development of land forms makes extremely difficult a valid quantitative and statistical approach, which demands that only like things be measured and compared. Surely, much remains to be learned through the analytical and qualitative approach. Some aspects of the work of winds, streams, and glaciers are fairly well understood, but an almost unlimited field remains for refinement as to

* This paper, illustrated with lantern slides, was presented at the meeting of the Section, March 5, 1951.
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processes and their effects, and as to the topographic effects of rock character and of structure.

If one doubts the above statement, let him try to formulate satisfactory answers to the following questions: 1. What causes an "open" valley like that of the Ohio river in its course from Pittsburgh to Louisville? 2. What is the relation between the load and the meandering of a stream? 3. Why are delta distributaries relatively straight? 4. What controls the texture of a drainage net? 5. What limits the size of ingrown meanders? 6. What is the relative importance of creep *vs.* sheetwash in interstream degradation in a moist climate? 7. How, in detail, does a scarp retreat, and how are the forms developed during that retreat influenced by the nature of the rock which caps the scarp? 8. Under what conditions do slopes retreat parallel to themselves? 9. How are even-crested ridges formed and maintained? 10. How are various kinds of rock expressed topographically?

The Etching Concept. In the past, land sculpture seems to have been thought of as almost exclusively the work of streams—the concept of stream sculpture. That process, of course, is important and, in some regions, is dominant, but for those regions underlain by diverse rocks and structures the writer believes that development of the etching concept offers a clear road to progress. That concept may be stated as follows: wherever the earth's crust is composed of diverse materials varying in resistance to weathering and corrasion, and lying in various attitudes, differential weathering, followed by removal of the weathered products, mainly by sheetwash and creep (except in arid lands where winds are important), so etches the surface that areas underlain by the more resistant rocks are brought into relief as the less resistant are lowered. Even small differences in structure and composition are thus revealed for interpretation by the geomorphologist.

A fundamental basis for the etching concept is the extreme contrast in rate of weathering exhibited by different rocks. Some waste away very quickly, others extremely slowly. Herein lies a possible field of investigation for those interested in the quantitative approach to geomorphic problems. Here also is a fundamental reason why much of the geomorphology of the past has been ineffective. Far too little consideration has been given to the nature of the underlying rocks.

Differential weathering is extremely selective. Wherever the products of weathering can be removed as rapidly as formed, even slight differences in resistance are expressed in the topography. But even where removal is slow, the soil cover deep, and vegetation dense, the weak rocks still form lowlands and the resistant rocks stand in relief.

Under the etching concept of erosion, interstream degradation is recognized as being of extreme importance. Streams serve mainly to cut down narrow notches which expose the rocks to the other agencies; they cut laterally to some extent; and they serve as sewers to carry away the material delivered to them by creep and sheetwash. They directly affect only a small proportion of the total land surface.

The influence of creep in moist, vegetation-covered areas seems to have been much underestimated. In such areas, it is by far the most effective agency for the removal of the products of weathering, whereas sheetwash and deflation play the leading roles in arid regions.

Geomorphic Interpretation of Rock Character. On the basis of their topographic expression, two major categories of rocks can be distinguished—the homogeneous and the heterogeneous. Rocks of either category may be weak or resistant.

Homogeneous rocks almost invariably show a dendritic drainage pattern, without evidence of structural control of the land forms. Permeable rocks tend to produce a coarse-textured topography, while the tex-

ture developed on impermeable rocks is fine because much of the rainfall, instead of joining the ground water, runs off and forms numerous small valleys.

Examples of homogenous rocks are coarse igneous intrusives and homogeneous shales. Both have dendritic drainage. Intrusives may be relatively weak, and form lowlands, or they may be resistant and stand high. Coarse, homogeneous, intrusive rocks generally weather into large rounded boulders which lie scattered over the surface. Shales as a rule are weak and are expressed as lowlands. The drainage net on them is closely spaced. Homogeneous sandstones, however, tend to develop coarse texture because of their permeability and their susceptibility to sheetwash rather than to gullying.

Heterogeneous rocks include the bedded sedimentary rocks having alternately resistant and weak beds, the lavas, and the metamorphic rocks. Sedimentary rocks whose strata are alternately weak and resistant are distinguishable by cliff and slope topography, and, as a rule, by uniformity of bedding. Lavas, on the contrary, show a rough bedding related to successive individual flows, but the bedding generally is much less distinct than that of sedimentary rocks, as well as thicker and more irregular.

Metamorphic rocks may, of course, be either of igneous or sedimentary origin. Metamorphic sedimentary rocks are expressed in topography somewhat in the same way as their unmetamorphosed equivalents, but they are generally more resistant and almost invariably are cut by numerous faults and shatter belts which are clearly expressed in the topography. This combination of intense fracturing with resistance to erosion leads to an irregularity and asperity of topography which is characteristic of most metamorphosed sedimentary rocks. In the old-age stage of the cycle, this is expressed as a peculiar "beaten silver" effect best seen from the air when the sun is low.

Metamorphic igneous rocks differ in expression from the unmetamorphosed mainly in a greater prominence of the effects of fracturing and also, in many instances, by vague indications of schistosity or gneissic banding.

Geomorphic Expression of Rock Attitude. In accordance with the etching concept, the attitude of hetereogeneous rocks should be very delicately registered in the topography, and we find that to be true. Horizontal strata of alternately weak and resistant materials are readily recognized by cliff and slope topography following a contour-like pattern. Alternately weak and resistant strata, if dipping gently, form cuestas of large or small scale. If dipping steeply, they form hog-back ridges and intervening weak-rock lowlands with drainage following the well-known trellis pattern.

The degree of dip is readily determined from the topography, and the direction of dip is easily found by noting the patterns of the resistant ledges as they cross stream valleys. If the dip exceeds 30 degrees, however, the hog-back ridges become symmetrical on the two sides and the direction of dip is then indeterminable, unless individual beds are revealed by streams cutting through the ridges.

Ridges on Resistant Rocks. Hard-rock ridges having relatively even crest lines have been much used in the interpretation of diastrophic history, but their dependability for such use is doubtful. Analysis of the processes at work in the denudation of such ridges will make it clear why this is so (see Rich, 1934).

Let us assume that a bed of resistant rock stands vertical and is flanked by a wide belt of weak rock on either side. The resistant rock, as it weathers, sheds resistant debris which tends to slide or creep down over the slopes of weak rock adjacent to the resistant bed, thus serving as a protective cover for them. If the hard-rock waste mantle were absent, the weak rock on which it lies would quickly weather and be lowered and carried away by creep and sheetwash, leaving the resistant bed

standing up like a wall, which would weather rapidly, being unprotected. If the resistant bed, on weathering, yields only sand-sized debris, with no protective capacity, such a wall-like outcrop would develop, but, in most instances, the hard rock breaks into coarse debris which blankets and protects the flanking weak rock, forming angle-of-repose slopes on both sides. The length of blanketed slope which can be protected depends, therefore, on the amount of debris available and this, in turn, on the width of outcrop, which is also the thickness of the bed of resistant rock. Hence, the height of a ridge is controlled by the thickness of the resistant bed which forms it.

In view of the fact that our assumed ridge is flanked on either side by weak rocks, and in view of the further fact that the difference in the rate of erosion between weak rocks like shale and resistant rocks like hard sandstone is extremely great, the weak-rock belts on either side of the debris-flanked ridge would be worn down to a local peneplain before the hard-rock ridge would be much reduced. The debris slopes flanking the ridge on either side thus have as their base levels the relatively flat and graded surface developed on the weak rocks. It follows from this and from the observed tendency toward constant slope angles, maintained by graded waste slopes, that the height of the ridge formed by the resistant rock would be essentially the same throughout its length because it would be determined by the meeting of waste slopes of essentially constant angle rising from the graded plains on either side. The crest of the ridge would, on the average, incline toward the nearest transverse stream cutting the ridge only to the degree that the graded trellis tributary streams, and the erosional local peneplain surface related to them, decline in that direction. The gentle inclination of the crests of Appalachian ridges toward the transverse streams which cut them thus has no real relation to those streams. The thicker the bed of resistant rock which forms the ridge, the higher the ridge. Its crest would be relatively even and would not necessarily imply peneplanation at or near the level of the ridge crest. Even-crested ridges, therefore, are not reliable criteria of peneplanation.

Following the same principle, a cuesta formed by a bed of resistant rock of a given thickness should stand higher than a hog-back ridge produced by the same bed where it stands vertical. All of its talus is available to maintain the slope on only one side and, therefore, a longer slope can be protected than would be possible for a hog-back ridge where the available debris must be divided between two opposing blanketed slopes.

Interpretation of Diastrophism by Means of Peneplains and Old-age Surfaces. A peneplain or an old-age surface implies a long still-stand of the land near base level. When a peneplain or an old-age surface stands high above any near-by base level, that fact implies an uplift since its formation. The degree of its dissection offers a measure of the time since uplift.

In using peneplains or old-age surfaces as evidences of diastrophism, we must be sure that those surfaces which appear to be peneplains are really such and are not ancient surfaces of marine planation resurrected by the stripping off of their sedimentary covers. In a surprising number of instances, it has been found that what were once thought to be highly uplifted peneplains are actually resurrected surfaces lying beneath Paleozoic marine rocks. We may cite as an example the highest "peneplain" on the summit of the Beartooth Mountains, which has been found to be overlain by Cambrian sedimentary rocks still remaining in patches over its surface. This striking erosional surface has no diagnostic significance therefore in connection with post-Early Paleozoic diastrophism.

On the Rocky Mountain Front Range of Colorado in the area mapped on the Pikes Peak folio (No. 7) of the U. S. Geological Survey, several areas which have been interpreted as peneplains formed since the

uplift of the Rocky Mountains are, instead, definitely resurrected surfaces, some of them beneath Early Paleozoic rocks and others beneath the Dakota sandstone of the Cretaceous. What are mapped on the folio as "Silurian" rocks stand on one of them as scattered buttes on the surface of the supposed peneplain. On another, remnants of basal Cretaceous rocks reveal that two smooth surfaces, which have been interpreted (Van Tuyl and Lovering, 1935) as two separate peneplains at different levels, actually represent post-Cretaceous warping and resurrection of the pre-Cretaceous plane of marine erosion (Rich, 1935).

Diastrophic Movements Rapid as Compared with Time Required for Formation or Destruction of Old-age Erosion Surfaces. A geomorphologist traveling and photographing in areas of recent pronounced crustal movement cannot fail, in such regions as the West Indies and parts of the Andes, to be struck by the clear evidences of warping, faulting, and folding of old-age erosion surfaces and by the relatively small alteration of those surfaces since their deformation. This feature is especially conspicuous on either side of the *Cul de Sac* of Haiti and Santo Domingo, where an old surface, much faulted in its lower part, rises from the floor of the *Cul de Sac* toward the 10,000-foot crest of the southern range of Haiti and Santo Domingo.

Conclusion. Geomorphology is an extremely useful tool for the interpretation of regional geology as well as of earth history. Since the geomorphologist has taken to the air or to the use of aerial photographs, the practical usefulness of his science has been increased many fold, and it is bound to grow as criteria are sharpened with the etching concept in mind. Far from being a finished science, geomorphology presents great opportunities for the future, especially in the analysis of geomorphic processes and in the sharpening of criteria for the recognition of different kinds of rocks and their areal distribution. It seems to the writer that much more is to be gained in the present stage of the science by careful analytical work than by quantitative work, except in instances where the factors involved can be isolated into definite categories which can be given separate treatment. Analysis and deduction, followed by field checking of the results of the deductions, still offer promise of rich rewards.

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SECTION OF PHYSICS AND CHEMISTRY

MESONS AND NUCLEAR FORCES*

By HIDEKI YUKAWA†

It is now well known that there is an intimate connection between the structure of atomic nuclei and the existence of a family of particles known as *mesons*. Although we do not yet have the final answer to the questions relating to mesons and nuclei, we notice that we have come nearer to the goal in the remarkable progress in nuclear physics since 1932, when the neutron was discovered. Up to that time, physicists had supposed that the atomic nucleus consisted of protons and electrons. If we start from such a model, however, and apply quantum mechanics, which

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proved to be so successful in the entire field of atomic physics, to problems of the nucleus itself, we encounter immediately a number of serious difficulties. For example, we know from the analysis of the molecular spectra that the nitrogen nucleus $_7N^{14}$ has spin 1 in units of $\hbar$ and obeys Bose-Einstein statistics, whereas the above model predicts that it should have half integer spin and obey Fermi-Dirac statistics, because it consists of an odd number of particles, *i.e.*, 14 protons and 7 electrons, each of which has spin $\frac{1}{2}$ and obeys Fermi-Dirac statistics. All this and similar difficulties disappeared at once when physicists adopted the new hypothesis that atomic nuclei consisted of *nucleons*, *i.e.*, protons and neutrons, both of which had spin $\frac{1}{2}$ and obeyed Fermi-Dirac statistics. Thus, for instance, $_7N^{14}$ consists of an even number of nucleons, *i.e.*, 7 protons and 7 neutrons.

On the other hand, however, the necessity of introduction of specific nuclear forces, which could not be reduced to electromagnetic forces between charged particles, became more obvious. Even in the older model for the nucleus, the electrostatic attraction between electrons and protons turned out to be much too small to account for the enormous binding energy of the nucleus. For example, the binding energy of the alpha-particle $_2He^4$, which was supposed to consist of four protons and two electrons, was more than 50 in units of $m_ec^2 = 0.5$ MeV, where m_e is the rest mass of the electron, while the electrostatic energy between a proton and an electron amounted only to m_ec^2 at a distance e^2/m_ec^2 or 2.8×10^{-13} cm. If we adopt the new model, the arguments for the existence of binding forces of non-electromagnetic origin between nucleons become more convincing, because protons must repel each other electrically, while neutrons are electrically neutral. Both the proton and the neutron have magnetic moments, but they are much too small to contribute appreciably to the nuclear binding.

It is now clear that the specific nuclear forces are to be effective only when two nucleons are at a distance of a few times of 10^{-13} cm. or less from each other. As a matter of fact, since 1911, when Rutherford had concluded the nuclear structure from the scattering of alpha-particles by atoms, it has been recognized that the Colomb repulsion was the only predominant force between an alpha-particle and a nucleus, unless they came to each other at a distance of the order of 10^{-12} cm. If we apply quatum mechanics to the new model for the nucleus, we find that the nuclear forces must have a very short range, of the order of 10^{-13} cm., in order to account for the rapid increase of the binding energy from 2.2 MeV for the deuteron to 28 MeV for the alpha-particle. On the other hand, the binding energies of nuclei heavier than the alpha-particle do not increase as rapidly as if they were proportional to the square of the mass number A, *i.e.*, the number of nucleons in each nucleus, but they are in fact approximately proportional to A itself. This indicates that nuclear forces are saturated for some reason. Heisenberg suggested a mechanism analogous to the chemical valence force. Namely, if we assume a force between a proton and a neutron, due to the exchange of the electric charge, for instance, the saturation of such a force can be concluded just as in the case of the chemical bond between a hydrogen atom and a proton due to the exchange of an electron. Soon afterwards, Fermi developed a theory of beta-decay based on the hypothesis by Pauli, according to which a neutron, for instance, could decay into a proton, an electron, and a *neutrino*, *i.e.*, a hypothetical neutral particle with a very small mass and an enormous penetrating power. This gave rise, in turn, to the expectation that nuclear forces could be reduced to the exchange of a pair of an electron and a neutrino between two nucleons, just as electromagnetic forces were regarded as due to the exchange of photons between charged particles. It turned out, however, that the exchange forces thus obtained were much too small, because the

beta-decay was a very slow process, whereas the extremely rapid exchange of the electric charge between nucleons was required as the mechanism of nuclear forces.

The idea of the meson field was introduced in 1935 in order to make up for this gap.[1] Original assumptions of the meson theory were as follows:

I. The nuclear forces are described by a scalar field U, which satisfies the wave equation

$$\left(\frac{\partial^2}{\partial x^2}+\frac{\partial^2}{\partial y^2}+\frac{\partial^2}{\partial z^2}-\frac{1}{c^2}\frac{\partial^2}{\partial t^2}-\kappa^2\right)U=0$$

in vacuum, where κ is a constant with the dimension of reciprocal length. When there is a nucleon, it becomes a point source of the field U of strength g, which is a constant with the same dimension as the electric charge.

By starting from this assumption, we can show that, when there are two nucleons at a distance r, the static interaction between them is given by a potential

$$+ \text{ or } - \; g^2 \exp(-\kappa r)/r,$$

so that the range of nuclear forces becomes $1/\kappa$. The numerical value of the constant g must be a few times larger than the elementary charge e, in order that the above potential is large enough to account for the binding energies of nuclei.

II. According to quantum theory, the field U is inevitably accompanied by a new type of particles or quanta, which have the mass

$$\mu = \frac{k\hbar}{c}$$

and the spin 0, obeying Bose-Einstein statistics. The mass μ can be inferred from the range of nuclear forces. If we take, for instance, $\kappa = 5 \times 10^{12}$ cm^{-1}, we obtain $\mu = 200\ m_e$.

Now, if we assume that U is a real function accompanied by neutral particles, *i.e.*, neutral mesons, we obtain ordinary attractive force of short range between two nucleons.

III. In order to obtain an exchange force between a proton and a neutron, we have to assume that U is a complex function accompanied by mesons with the electric charge $+e$ or $-e$ and that a positive (negative) meson is emitted (absorbed) when the nucleon jumps from the proton state to the neutron state, whereas a negative (positive) meson is emitted (absorbed) when the nucleon jumps from the neutron state to the proton state. The mechanism of the exchange force between a proton and a neutron is thus reduced to the exchange of charged mesons just as two charged particles interact electromagnetically by exchanging photons.

In this way, we obtain an exchange force of Heisenberg type with the appropriate range and magnitude. The force thus obobtained, however, turned out to have the wrong sign, in that it was repulsive for triplet S-state of the deuteron in contradiction to the experiment. Moreover, we could not derive the exchange force of Majorana type, which was necessary in order to account for the saturation of nuclear forces just at the alpha-particle, while the exchange force of Heisenberg type was to be saturated already at the deuteron. In order to remove these defects, more general types of meson fields, including vector, pseudoscalar, and pseudovector fields, in addition to the scalar field, were considered by various authors.[2] In particular, the vector field was investigated in detail, mainly because it could give a combination of exchange forces of Heisenberg and Majorana types with correct signs and could further account for the anomalous magnetic moments of the neutron and the proton qualitatively. Furthermore, the vector theory predicted the existence of noncentral forces between a neutron and a proton, so that the ground state of the deuteron was to be a mixture of triplet S and triplet D states. This implied, in turn, that the charge distribution in the deuteron was not spherically symmetric, but the density was to be larger in the direction which was perpendicular to the direc-

tion of the resultant spin. In other words, the electric quadripole moment of the deuteron was to be negative. According to the experiment by Rabi and others,[3] however, it was known to be positive. For this reason, the vector theory finally gave place to the pseudoscalar theory, which gave the correct signs for both the nuclear forces and the quadripole moment of the deuteron.[4] In any case, it is noticeable that the non-central part of the nuclear potential, which is usually called tensor force, is of the same order of magnitude as the central force, whereas, in the case of the electromagnetic interaction, the central force which was derived from the Coulomb potential was predominant.

There was another feature of nuclear forces, which was to be accounted for in some way or other as the consequence of meson theory. Namely, the results of experiments on the scattering of protons by protons at high energy up to a few MeV indicated that the type and magnitude of forces between two protons were, at least approximately and at least in singlet S-state, the same as those between a proton and a neutron, apart from the Coulomb force in the former case. Now, the interaction between two protons or two neutrons was obtained only if we took into account the terms proportional to g^4, whereas that between a neutron and a proton was proportional to g^2, as long as we were considering charged mesons alone. The terms proportional to g^4 are, in general, smaller than those proportional to g^2, unless the coupling constant g is unusually large for some particular reason, as in the case of pseudoscalar meson field with pseudoscalar coupling. Even in the latter case, the type and range of forces which are proportional to g^4 are different from those of g^2 forces.

IV. It seems to be almost inevitable, therefore, to assume further that in addition to the charged mesons there are neutral mesons with a mass either exactly or approximately equal to that of the charged mesons. They must also have the integer spin, obey Bose-Einstein statistics, and interact with the nucleons about as strongly as the charged mesons.

This assumption obviously increased the number of arbitrary constants in meson theory, which could be so adjusted as to agree with a variety of experimental facts. These experimental facts could not be restricted to those of nuclear physics in the narrow sense, but were to include those related to cosmic rays, because we expected that mesons could be created and annihilated due to the interaction between matter and cosmic ray particles with energies much larger than μc^2, which was of the order of 100 MeV. In fact, the discovery of particles of intermediate mass in cosmic rays in 1937 by Anderson and Neddermeyer[5] was a great encouragement to further developments of meson theory. At that time, we came naturally to the conclusion that the mesons which constituted the main part of the hard component of cosmic rays at sea level were to be identified with the mesons which were responsible for nuclear forces.[6] Indeed, cosmic ray mesons had the mass around 200 m_e as predicted and moreover, there was definite evidence of the spontaneous decay of mesons, which was the consequence of the following assumption of the original theory:

V. Charged mesons interact also with light particles, *i.e.*, electrons and neutrinos, just as they interact with nucleons, the only difference being the smallness of the coupling constant g', in this case compared with g. Thus, a positive (negative) meson can change spontaneously into a positive (negative) electron and a neutrino.[7]

According to this assumption, the proper life-time, *i.e.*, the mean life-time at rest, of the charged scalar meson, for instance, is given by

$$\tau_0 = 2\,(\hbar c/(g')^2)\cdot(\hbar/\mu c^2).$$

When the mesons are moving with the velocity v, their mean life-time increases by a factor $1/\sqrt{1-(v/c)^2}$, due to the well-known relativistic delay of the moving clock. Although the spontaneous decay and the velocity dependence of the life-

time of cosmic ray mesons seemed to confirm the above assumption, there was an undeniable discrepancy between theoretical and experimental values for the proper life-time. The original intention of meson theory was to account for the beta-decay by combining assumptions III and V together. The coupling constant g', however, which was so adjusted as to give the correct result for the beta-decay, turned out to be too large, in that it gave the life-time τ_0 of the order of 10^{-8} sec., which was much smaller than the observed value 2×10^{-6} sec. There were indications, moreover, which were by no means in favor of the expectation that cosmic ray mesons interacted strongly with nucleons. For example, the observed cross-section of scattering of cosmic ray mesons by nuclei was much smaller than that obtained theoretically.

Thus, already in 1941, the identification of cosmic ray mesons with the mesons, which were supposed to be responsible for for nuclear forces, became doubtful. Tanikawa and Sakata[8] proposed in 1942 the following modification of meson theory: The mesons which constitute the hard component of cosmic rays at sea level are not directly connected with nuclear forces, but are produced in the upper atmosphere by the decay of heavier mesons which interact strongly with nucleons. We had to wait for a few years until 1947, however, when two very important facts were discovered which seemed to confirm the above *two-meson hypothesis*. First, Italian physicists[9] discovered that the negative mesons in cosmic rays, which had been captured by light atoms, were not absorbed instantly by nuclei, but very often decayed into electrons in a mean time interval of the order of 10^{-6} sec. This could be understood only if we accepted the assumption that ordinary mesons in cosmic rays interacted very weakly with nucleons. Soon afterwards, Powell and others[10] discovered two types of mesons in cosmic rays at the high altitude, the heavier mesons decaying in a very short time into lighter mesons. Just before the latter discovery, the two-meson hypothesis was proposed by Marshak and Bethe,[11] independently of the above-mentioned Japanese physicists. In 1948, mesons were created artificially for the first time in Berkeley,[12] and subsequent experiments on artificial mesons further confirmed the general picture of two-meson theory.[13]

The fundamental assumptions which are now substituted for the original assumption V are as follows:

(1) The heavier mesons, *i.e.*, π-mesons with the mass $m\pi$ around 276 m_e, interact strongly with nucleons and can decay into lighter mesons, *i.e.*, μ-mesons and neutrinos with a life-time of the order of 2×10^{-8} sec. π-Mesons have integer spin (very probably spin 0) and obey Bose-Einstein statistics. They are responsible for at least a part of nuclear forces. This is because the shape of nuclear potential at a distance of the order of $\hbar/m_\pi c = 1.4 \times 10^{-13}$ cm. or larger must be inseparably connected with the strong interaction of π-mesons with nucleons.

(2) The lighter mesons, *i.e.*, μ-mesons, with the mass about 210 m_e, are the main constituent of the hard component of cosmic rays at sea level and can decay into electrons and neutrinos with the life-time 2×10^{-6} sec. They very probably have spin $\frac{1}{2}$ and obey Fermi-Dirac statistics. As they interact only very weakly with nucleons, they have nothing to do with nuclear forces.

If we accept these assumptions together with assumption IV, the existence of the neutral π-meson in addition to the charged π-meson is naturally expected. Such neutral mesons, however, which have integer spin and interact as strongly as charged π-mesons with nucleons, must be very unstable, because each of them can decay into two or three photons in a very short time.[14] In particular, a neutral meson with spin 0 can decay into two photons, and the life-time is likely to be of the order of 10^{-14} sec. or possibly even less than that. Recently, it became clear that some of the

experimental results obtained in Berkeley[15] could be accounted for consistently by assuming that, in addition to charged π-mesons, neutral π-mesons with the mass approximately equal to that of charged π-mesons were created by high energy nuclear collisions and that these neutral mesons decayed into two photons with the life-time of the order of 10^{-13} sec. or less. Thus, the spin of neutral mesons must be 0. Other experiments in Berkeley are not only consistent with this assumption, but indicate further that both charged and neutral π-mesons are pseudoscalar, although the mass of the neutral π-meson turns out to be around 265 m_e, which is a little less than that of the charged π-meson.[16] Generally speaking, this is what we have expected theoretically, because the pseudoscalar meson theory was known to be the only one which could reproduce qualitatively all the required properties of nuclear forces.

If we go into details, however, we find that the situation is still very unsatisfactory. We could not yet arrive at the quantitative agreement between theory and experiment. There are a number of important problems, moreover, which must be intimately connected with the problems of π-mesons and nuclear forces, but are not yet fully investigated. Among other things, we do not know much about the mesons heavier than π-mesons, although recent experiments show beyond doubt the existence of very unstable mesons in cosmic rays, which are much heavier than π-mesons. We do not yet know whether some of the heavier mesons contribute appreciably to nuclear forces at very short distances. Furthermore, the present form of field theory including meson theory as a particular case is not free from the divergence difficulties. Meson theory has changed a great deal during these sixteen years. Nevertheless, we shall probably have to go through another revision before we shall finally arrive at the complete understanding of the nuclear structure and of a great variety of phenomena occurring in high energy region.

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SECTION OF BIOLOGY

SYMPOSIUM*

INDIVIDUAL BLOOD DIFFERENCES

Chairman: E. D. GOLDSMITH

Introduction *By* E. D. GOLDSMITH

The Rh-Hr Blood Types: Serology, Genetics, and Nomenclature. *By* A. S. WIENER

A Brief Review of the Newer Blood Factors. *By* PHILIP LEVINE

Erythrocyte-Iso-antibody Reactions in Dogs. *By* L. E. YOUNG, W. A. O'BRIEN, G. MILLER, S. N. SWISHER, D. M. ERVIN, R. M. CHRISTIAN, AND C. L. YUILE

The Identification of Hemagglutinins with Red Cells Altered with Trypsin. *By* R. E. ROSENFIELD AND P. VOGEL

Treatment of *Erythroblastosis fetalis* by Exchange Transfusion. *By* G. J. BRANCATO

INTRODUCTION

By E. D. GOLDSMITH†

About six years ago, in May of 1945, to be precise, a Conference on Blood Grouping‡ was sponsored by the Section of Biology of The New York Academy of Sciences. In opening the meeting at that time, it was emphasized that in the light of our experience with the life-saving value of plasma, plasma derivations, and whole blood during the Second World War, the importance of the subject was apparent to all.

Unfortunately, the all too familiar events of 1951, and the shadows of things which we hope will not come, have presented new and acute problems in the field of blood, involving the civilian population as well as the military. Following in the tradition of the late Doctor Karl Landsteiner, the research progresses, the new problems which are solved as they arise in turn produce new puzzles which await solution. With the discovery of additional differences, the situation becomes increasingly complex.

In this connection, I am reminded of Doctor R. R. Race's remarks§ following the paper by Doctors Wiener and Sonn, "The Rh Series of Genes, with Special Reference to Nomenclature." Doctor Race said in part: "...we have gone further than Wiener and much further than Levine has ventured. Wiener and Levine probably think we have gone so far that we are lost, and, of course, they may be right." What many of us would like to know is: "Are we on course and where are we going in this very important phase of blood research?"

Tonight, we are privileged to have papers presented by a number of the outstanding contributors to this field. My only regret is that time made it necessary to limit the number of speakers.

* Held on March 12, 1951.

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‡ Ann. N. Y. Acad. Sci. 46 (9): 883–992.

§ Ann N. Y. Acad. Sci. 46 (9): 988.

THE RH-HR BLOOD TYPES: SEROLOGY, GENETICS, AND NOMENCLATURE

By A. S. WIENER*

The progress of knowledge of the Rh-Hr types has been so rapid that few workers have been able to keep pace with the recent developments or to understand thoroughly the significance of the findings reported. The purpose of this presentation is to summarize the present status of the subject, in an attempt to dispel some of the confusion which prevails.†

In the original work of Landsteiner and Wiener,[1, 2] the first and most important Rh factor was discovered. Promptly thereafter, immune iso-antibodies were found in human sera which identified different blood factors related to the original rhesus factor.[3–5] At the present time, reagents for four Rh factors and two Hr factors are available in laboratories specializing in this work.[6, 7] The chronological development of this aspect of the subject is summarized in TABLE 1.

With the discovery of additional varieties of Rh and Hr antisera, the number of recognizable phenotypes increased from 2 to 8, and later to 18 or more, depending on the number of antisera used in the tests. As a result, there were rapid changes in the nomenclature, because names had to be coined for these phenotypes. As was to be expected, many armchair philosophers offered suggestions for other systems of nomenclature, but, luckily, all but one of these has fallen by the wayside. The suggestion of Fisher and Race[23] to use the letters C, D, and E, to designate the blood factors rh′, Rh_0, and rh″, respectively, and the small letters c, d, and e for the re-

TABLE 1

EVOLUTION OF KNOWLEDGE OF THE RH-HR FACTORS

I. *Anti-Rh Sera*

(*a*) Anti-Rh_0—Sera of this specificity first discovered by Landsteiner and Wiener[1, 2] (1937–40) by immunizing rabbits and guinea pigs with rhesus blood. Human anti-Rh_0 sera identified by Wiener and Peters[8] (1940) in patients having hemolytic transfusion reactions, and by Levine *et al.*[9, 10] (1940–41) in mothers of erythroblastotic babies. This Rh antibody is the most common cause of clinical problems, and thousands of sera of this specificity have been found or produced.

(*b*) Anti-rh′— First identified by Wiener[3, 11] (1941) in a patient having an hemolytic transfusion reaction. Found later the same year by Levine *et al.*[10] in serum of mothers of erythroblastotic babies. Many examples of this antibody have since been found, most commonly in association with anti-Rh_0.

(*c*) Anti-rh″— First described by Wiener and Sonn[4, 12] (June, 1943) and additional examples found by Race *et al.*[13] later the same year. Like anti-rh′, this antibody usually occurs in association with anti-Rh_0.

(*d*) Anti-rh^w— First encountered by Callender, Race, and Paykoc[14] (1946) in a patient with lupus erythematosus who developed multiple sensitizations following blood transfusions. Additional examples of this rare antibody later found by van Loghem[15] and Broman.[16]

II. *Anti-Hr Sera*

(*a*) Anti-hr′— First serum with reactions reciprocally related to Rh found by Levine and Javert[10, 17] (1941–43), but reactions weak. Stronger serum found by Race and Taylor[18] (1943) and properties fully described. A third example was found by Wiener, Davidsohn, and Potter[19] (1944–45), who showed that all three sera were of specificity anti-hr′. Many other examples of this antibody have since been encountered.

(*b*) Anti-hr″— First example of this antibody was found by Mourant[20] (1946). A second example was found by Wiener and Peters[21] (1948). Only a few additional examples of this rare antibody have since been encountered by other workers.

(*c*) Anti-Hr_0— Several claims regarding this antibody have been published, but no usable reagent has ever been prepared. Wiener,[22] therefore, doubts that this antibody has really been found to date.

III. *Rh Variants*

First described by Wiener (1944). Symbols C^w, C^u, c^v, and D^u introduced by British investigators (1946–50).

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† The status of the subject five years ago was summarized in a previous presentation before the Academy (WIENER, A. S. & E. B. SONN. 1946. Ann. N. Y. Acad. Sci. 46: 969–972.)

ciprocally related Hr factors acquired widespread usage, especially after Cappell suggested anti-C, anti-D, *etc.*, for the corresponding antibodies, instead of the Greek letters used by Fisher. The British notations, besides lacking priority, incorrectly imply a relationship to the A-B-O system and become cumbersome or impossible to use orally when describing the complex

TABLE 2

NOMENCLATURE OF THE RH-HR BLOOD FACTORS

Rh Factors		*Hr Factors*	
International notations	*British notations*	*International notations*	*British notations*
Rh_0	D	(Hr_0)	(d)
rh′	C	hr′	c
rh″	E	hr″	e
rh^w	C^w		

Rh-Hr agglutinogens and phenotypes. The most important objection to the notations is that they are based on an incorrect genetic theory and convey fallacious concepts concerning the serology of the Rh-Hr blood types.[24–30] For these reasons, the C-D-E notations are falling into disuse.

In TABLE 2, the equivalent symbols for the Rh-Hr factors according to the International Nomenclature and British notations* are presented, and, in TABLE 3, these notations are applied to the eight Rh phenotypes which can be distinguished by tests with the three "standard" Rh antisera. The International Nomenclature is simple and logical, and is quickly understood and learned. All blood types containing the most important Rh factor, Rh_0, are indicated by a capital "R," while those lacking the Rh_0 factor are indicated by a small "r." If one understands the four blood groups, O, A, B, and AB, the scheme is easy to learn. One has only to learn a few simple abbreviations such as Rh_1 for Rh_0', and Rh_2 for Rh_0''. On the other hand, it is difficult to designate the phenotypes satisfactorily in terms of C-D-E, so that no uniform method has been adopted by workers using these symbols, and only a few of the many methods used are shown in the table.† Race[31] and Fisher have used two methods (among others), namely, either by indicating the reactions with the antisera with plus and minus signs, or by adopting the symbol for

TABLE 3

THE EIGHT RH BLOOD TYPES

Designations of phenotypes (International Nomenclature)	*Reactions with antisera*			*Methods of designating phenotypes, used by workers favoring the British notations*			
				Fisher-Race		*Mourant-Bessis*	*Matson, Levine, and others*
	Anti-rh′	*Anti*-rh″	*Anti*-Rh_0	*C-D-E*	*Most likely genotype*		
rh	−	−	−	−−−	cde/cde	ccddee	cde
rh′	+	−	−	+−−	Cde/cde	Cddee	Cde
rh″	−	+	−	−−+	cdE/cde	ccddE	cdE
rh_y (or rh′rh″)	+	+	−	+−+	Cde/cdE	CddE	CdE
Rh_0	−	−	+	−+−	cDe/cde	ccDee	cDe
Rh_1	+	−	+	++−	CDe/cde	CDee	CDe
Rh_2	−	+	+	−++	cDE/cde	ccDE	cDE
Rh_z (or Rh_1Rh_2)	+	+	+	+++	CDe/cDE	CDE	CDE

the most likely genotype to represent the phenotype. The objections to either method are so manifest even to Fisher[32] and Race[33] that, whenever necessary to make themselves understood, they use so-called "shorthand" symbols R_1, R_2, *etc.*, which are merely minor modifications of the International Nomenclature, and thus place themselves in the position of arguing that two or three nomenclatures are simpler to use than one. While the symbols suggested by Mourant[34] and Bessis are the most logical, they have not been widely adopted, because of their cumbersome nature. All the methods of designating

* One can hardly dignify the British notations by calling them a nomenclature, considering that they make no adequate provision for designating phenotypes (*cf.* TABLE 3).

† The extremely cumbersome method of bracketing together all possible genotypes to represent the phenotype could not be shown in TABLE 3 for lack of room. For example, type Rh_1Rh_2 would have to be represented by 14 genotypes bracketed together (*cf.* TABLE 4).

TABLE 4

COMPARISON OF THE INTERNATIONAL RH-HR NOMENCLATURE AND THE BRITISH NOTATIONS AS APPLIED IN GENETIC STUDIES

Mating	*Types possible in children*	*Types impossible in children*			
rh × rh	rh	rh′, rh″, rh_y, Rh_0, Rh_1, Rh_2, Rh_z			
cde/cde × cde/cde	cde/cde	Cde/Cde Cde/cde	cdE/cdE cdE/cde	Cde/cdE CdE/cde CdE/Cde CdE/cdE CdE/CdE	cDe/cDe cDe/cde
		CDe/cde Cde/cDe CDe/cDe CDe/CDe CDe/Cde	cDE/cde cdE/cDe cDE/cDe cDE/cDE cDE/cdE	CDe/cDE CDe/cdE Cde/cDE CdE/cDe CdE/CDe CdE/cDE CDE/cde CDE/Cde CDE/cDe CDE/cdE CDE/CDe CDE/cDE CDE/CdE CDE/CDE	

TABLE 5

STATISTICAL ANALYSIS OF DATA ON THE DISTRIBUTION OF THE RH-HR TYPES IN LONDON

Rh-Hr phenotypes	*Number*	*%*
rh	170	15.84
rh′rh′	0	0
rh′rh	10	0.93
rh″rh″	0	0
rh″rh	7	0.65
rh_yrh rh_yrh' rh_yrh'' rh_yrh_y	0	0
Rh_0	19	1.77
Rh_1Rh_1	190	17.71
Rh_1rh	363	33.83
Rh_2Rh_2	29	2.70
Rh_2rh	137	12.77
Rh_1Rh_0	144	13.42
Rh_zRh_1	4	0.37
Rh_zRh_2 Rh_zRh_z	0	0
Totals	1073	99.99

r = 39.80%
r' = 1.15
r'' = 0.81
R^0 = 2.19
R^1 = 40.57
R^2 = 15.28
R^z = 0.44

$\Sigma R = 100.24$

$\sqrt{(rh'-)} + \sqrt{(hr'-)} = 100.59\%$
$\sqrt{(rh''-)} + \sqrt{(hr''-)} = 100.11\%$

phenotypes by the symbols C-D-E have the serious disadvantage that they fail to indicate the special position of the Rh_0 factor, in contrast to factors rh′ and rh″. The situation becomes even more compli-

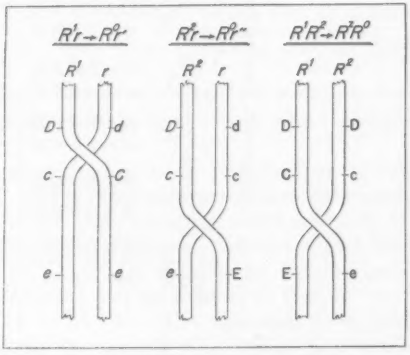

FIGURE 1. Fisher-Race cross-over hypothesis.

cated when the Hr antisera and Rh variants are taken into account, but at this point we must first discuss the genetics of the Rh-Hr types.

Based on earlier work on the A-B-O groups and M-N types, it was obvious to me[35] from the outset, when only factors

TABLE 6

STATISTICAL TEST OF FISHER-RACE CROSS-OVER HYPOTHESIS

Population	*Investigator*	*Number of persons tested*	*Calculated frequencies (%) of genes ("chromosomes")*							*Calculated frequency (%) of genotypes*			*"Chromosome" segments*		
			r	r'	r''	R^0	R^1	R^2	R^z	R^1R^2	R^1r	R^2r	*C-E* $\left(=\frac{R^z}{R^1R^2}\right)$	*D-C* $\left(=\frac{r'}{R^1r}\right)$	*D-E* $\left(=\frac{r''}{R^2r}\right)$
Caucasoids															
U. S. A.	Wiener *et al.*	2,390	36.65	1.23	0.52	3.73	42.70	15.06	.05	12.86	31.30	11.04	.004	.039	.045
U. S. A.	Unger *et al.*	7,317	38.3	1.4	0.8	2.8	42.1	15.1	—	12.71	32.25	11.57	—	.043	.069
England	Race *et al.*	2,000	28.86	0.98	1.19	2.57	42.05	14.11	0.24	11.87	32.68	10.97	.020	.030	.108
Canada	Chown *et al.*	3,100	39.55	1.24	0.73	1.91	43.48	12.87	0.22	11.19	34.39	10.18	.020	.036	.072
Spain	Race *et al.*	223	36.95	0	0.61	0.61	50.11	12.16	0.45	12.19	37.03	8.99	0	0	.051
Czechoslovakia	Raska *et al.*	181	40.02	0.69	0.69	1.36	39.59	16.90	0.75	13.38	31.68	13.52	0	.022	.051
Basques	Chalmers *et al.*	383	53.16	1.47	0.25	0.50	37.56	7.07	0	5.31	39.93	7.51	0	.037	.033
Negroids, N. Y. C.	Wiener *et al.*	223	28.4	2.7	0	42.1	11.7	14.4	0	3.37	6.65	8.18	0	.41	0
Pygmies, Belgian Congo	Wiener *et al.*	200	27.39	0.89	0.89	43.32	17.30	9.02	0	3.10	9.54	4.89	—	.093	.182
Siamese	Hubinont and Snoeck	94	10.5	0	0	63.2	6.2	19.5	0	2.4	1.3	4.1	0	0	0
Papuans	Phansanboom *et al.*	213	0	0	0	11.13	75.54	11.13	2.16	16.82	0	0	.128	—	—
Australian aborigines	Simmons and Graydon	100	0	0	0	2.1	94.3	2.0	1.6	3.8	0	0	.42	—	—
New Caledonians	Simmons and Graydon	234	0	12.87	0	8.54	56.42	20.09	2.08	22.67	0	0	.091	—	—
Mexican Indians	Simmons *et al.*	325	0	0	0	5.48	83.32	10.77	0.43	17.95	0	0	.025	—	—
Navaho Indians	Wiener *et al.*	98	0	0	0	5.8	64.1	26.8	3.3	34.36	0	0	.096	—	—
	Boyd and Boyd	305	0	17.31	2.02	8.37	31.05	35.33	5.73	21.9	0	0	.262	—	—

Rh_0 and rh′ were known, that one had to consider the alternative possibilities of whether the types determined by these Rh factors were transmitted by multiple allelic genes, each of which gave rise to a complex agglutinogen, or whether there were separate pairs of allelic genes for Rh_0 and rh′, either in different chromosomes or linked within the same chromosome. If there were separate genes for Rh_0 and rh′, either linked or independent, then at equilibrium $(Rh_0-/rh'-) \times (Rh_0+/rh'+) = (Rh_0+/rh'-) \times (Rh_0-/rh'+)$. The fact that the equality does not hold disproves the hypothesis of linked or independent gene pairs.[35, 36] Therefore, I adopted the theory of multiple alleles to account for the heredity of the Rh-Hr blood types.[3–5, 37, 38] Subsequently, however, Fisher and Race[23, 39] revived the linkage theory discarded by me and elaborated it to include the rh″ factor.

Direct evidence in support of the theory of multiple alleles has been obtained by investigating the Rh-Hr types in families,[40–51] and by mathematical analysis of statistical data on the distribution of the Rh-Hr blood types in different populations.[40, 52, 53] As an example of the latter, in TABLE 5 I have analyzed Race's data on the distribution of the Rh-Hr blood types among 1,073 randomly selected individuals in London. On the other hand, there is no direct evidence in support of the concept of linked genes. Instead, as already pointed out, there is statistical evidence refuting it. Moreover, a search for families showing crossing over between the supposedly linked genes C-D-E has been fruitless.[33]

Fisher and Race [54] have invoked the following ingenious but fallacious argument in favor of the cross-over hypothesis. According to them, in Caucasoids the less common genes R^0, r', r'', and R^z arose from genes R^1, R^2, and r by crossing over, as shown in FIGURE 1. On the basis of this theory, they assert that gene r^y is the rarest of the genes because it could arise only by double crossing over. Moreover, each time that r', r'', or R^z arise by crossing over, R^0 must also be formed, so that $R^0 = r' + r'' + R^z$. This relationship, in fact, does seem to hold fairly well for Caucasoids in England* (*cf.* TABLE 6).[54] In addition, Fisher and Race estimated the distances between the hypothetical genes C-D-E by taking the ratio of each rare gene to the genotype from which it is supposed to have arisen. Based on the calculation for British, they arranged the genes in the order

TABLE 7

THE BLOOD FACTORS PRESENT IN THE RH AGGLUTINOGENS

Wiener notations		*Reactions with antisera*						*Fisher-Race notations*	
Gene	*Corresponding agglutinogen*	rh′ C	rh^w C^w	rh″ E	Rh_0 D	hr′ c	hr″ e	*Designations**	*Error*
r	rh	−	−	−	−	+	+	*cde*	*d* not demonstrated
r'	rh′	+	−	−	−	−	+	*Cde*	*d* not demonstrated
r''	rh″	−	−	+	−	+	−	*cdE*	*d* not demonstrated
r^y	rh_y	+	−	+	−	−	−	*CdE*	*d* not demonstrated
R^0	Rh_0	−	−	−	+	+	+	*dDe*	None
R^1	Rh_1	+	−	−	+	−	+	*CDe*	None
R^{1w}	Rh	+	+	−	+	−	+	*C^{w}De*	*C* omitted; four factors present
R^2	Rh_2	−	−	+	+	+	−	*cDE*	None
R^z	Rh_z	+	−	+	+	−	−	*CDE*	None
R^{1y}	Rh_1^y	+	−	−	+	+	−	*c^{y}De*	Symbol does not indicate presence of *C* and *c*†
New rare agglutinogen of Race and Sanger		−	−	−	+	−	−	*-D-*	Contains only one factor

* The Fisher-Race nomenclature does not distinguish between genes (chromosomes) and agglutinogens, or between genotypes and phenotypes.

† Moreover, this contradicts the Fisher-Race theory, which precludes the presence of both *C* and *c* in the same chromosome and agglutinogen.

* But, when the gene frequencies are corrected for the Rh_0 variants, the relationship no longer holds.

D-C-E and showed that the distance D-E = D-C + C-E. As shown in TABLE 6, however, when the same calculations are applied to other populations, these relationships no longer hold, indicating that the results obtained by Fisher and Race were accidental (*cf.* Ottensooser and Pasqualin[58]). On the other hand, the calculated sum of the gene frequencies always approximates 100 per cent, as required by the theory of multiple alleles.

It should be pointed out that the sponsors of the cross-over hypothesis have recently exhibited a tendency to force the facts to fit their preconceived ideas. For example, the designations anti-C + C^w and anti-D + D^u used by Race[56, 57] are misleading, if not incorrect, because both of these sera contain only a single antibody.[58] On the other hand, the symbol C^wDE fails to indicate the presence in blood of this type of both elementary antigens, C and C^w, while the symbol c^vDe does not indicate the reaction of this agglutinogen with both antisera, anti-C and anti-c. Moreover, the Fisher-Race theory precludes the existence of chromosomes and agglutinogens containing both properties C and c at the same time. This and other fallacies of the C-D-E notations are summarized in TABLE 7.

In conclusion, therefore, the evidence available at the present time supports the theory of multiple alleles for the Rh-Hr types, and refutes the cross-over hypothesis of Fisher and Race. The universal adoption of the original International Rh-Hr Nomenclature and the elimination of the incorrect C-D-E notations will help eradicate the prevailing confusion concerning the subject.

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A BRIEF REVIEW OF THE NEWER BLOOD FACTORS

By PHILIP LEVINE*

The discovery of transplacental iso-immunization with its relationship to hemolytic disease, the ever increasing use of blood transfusions, and the elaboration of new methods for detection of intragroup iso-immunization has led to the description of a variety of new blood factors so that the Landsteiner concept of the individuality of human blood is now fulfilled. This highly specialized field has become so involved by virtue of the numerous blood factors that workers in charge of transfusion services and blood banks have become unduly apprehensive about possible sensitization and hemolytic reactions in patients who may be candidates for a series of transfusions. Fortunately, not all blood factors have equal clinical significance either in hemolytic disease or transfusions, and sensitization to many of the new blood factors is remarkably rare.

A review of the newer blood factors in the limited space available can best be presented by a classification of at least three varieties of iso-agglutinins employed for their detection. Among the more important criteria which permit a classification are the effect of temperature, range of titer, and the degree of clinical importance in hemolytic disease and in transfusions. These and several other differentiating features of three different varieties are presented in TABLE 1.

Difficulties, however, are encountered in attempting to enumerate the several antibodies in each of the three groups, because different specimens of the same antibody, *e.g.*, anti-M and anti-S, have been observed which belong to each of the three different groups. Another consideration is the lack of complete data for the rare antibodies, in several of which only one sample has thus far been recorded.

Historically, it is of interest that the term "atypical agglutinins" as a class of naturally occurring antibodies was defined in 1929 by Landsteiner and Levine[1] in their comprehensive studies of normal human sera. In the light of recent developments, it is advisable to abandon the term "atypical agglutinins" and to refer to them by their most significant characteristic, *i.e.*, "naturally occurring" antibodies. Because of their low titers, inactivity or almost complete loss of activity at 37°C., and uneventful transfusion of sensitive blood in five patients, it became necessary to separate them from anti-A and anti-B, at the time the only clinically important antibodies. In addition to anti-A_1, anti-A_2, and anti-P, five samples of an unnamed agglutinin were studied which gave about 16 per cent distinct positive reactions at 20°C. A review of some of their properties, such as activity at 37°C., although to a somewhat weaker degree, their peculiar formation of large loose clumps with many free cells, and a peculiar heredity leave no doubt that these five sera of 1929 contained antibodies identical to that of anti-Lewis (anti-Le^a), independently described by Mourant[2] in 1946.

The term "warm agglutinins" was first applied by Levine[3] in 1940 to the then unnamed (later identified as anti-Rh) intragroup agglutinins shown to be responsi-

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ble for hemolytic transfusion reaction in women at the first transfusion and somewhat later associated with hemolytic disease of the newborn. Without exception, this group of antibodies is "immune" in origin and includes all antibodies causing hemolytic disease and almost all antibodies associated with hemolytic transfusion reactions. Since the term "warm agglutinins" was applied prior to the description of incomplete antibodies (albumin agglutinins), the indirect Coombs test, and the use of trypsinized cells, the more comprehensive term, "warm immune agglutinins," was chosen for all the antibodies listed under this heading in TABLE 1, including the rare instances of hemolysins.

The Rh-Hr Factors. Only a few remarks need be made regarding these blood factors. It should be pointed out that of the several variants of this system only the factor C^w can be recognized by a specific antibody. Specific antibodies for the others, C^v, C^u, D^u, E^u, do not exist, and their demonstration depends on quantitative effects in studies with several specimens of the same antibody. Anti-C^w frequently occurs along with anti-C, but it has been reported as the only antibody produced by a mother who delivered an infant with hemolytic disease.[4] Anti-C^w acts on about 2.5 per cent of random bloods.

The D^u factor in its numerous forms can be considered as a modified D antigen, which reacts weakly or not at all when tested with some anti-D sera. In order to detect this variant, it is necessary to subject all bloods selected as Rh negative in the initial screening to an additional test, *i.e.*, exposure to potent albumin agglutinins followed by the indirect Coombs test. From a practical standpoint, the D^u variant should be considered just as antigenic as the normal D factor.

TABLE 1

CLASSIFICATION OF HUMAN ISO-ANTIBODIES

	Naturally occurring	*Intermediate*	*Warm immune**
Optimum temperature	18°–20°C.	20°–37°C.	37°C.
Titer	low	intermediate	high
Antigenic response	weak, rare	weak, occasional	distinct
Positive antiglobulin test	negative	occasional	always†
Clinical importance			
1. Transfusion	none	rare	important
2. Hemolytic disease	none	not proven	important
Use in paternity disputes	none	selected sera only	very useful
Blood factors	A_1, A_2, P Le^a, Le^b, M, N	Lu^a, S, M, N, Gr	Rh-Hr system Kk, Fy^a, S, Jk^a, M N (rare), Tj^a

* Includes rare sera containing hemolysins.
† With incomplete antibodies.

An analysis of the properties of several agglutinins makes it necessary to include another group which is intermediate between the two above-mentioned varieties. Not one case of hemolytic disease could be attributed to any of these antibodies and their initial role in hemolytic transfusion reactions is probably not very important. These antibodies are frequently associated with other antibodies of the warm immune type. In other instances, the properties of naturally occurring antibodies may be altered by the antigenic stimulus of a transfusion so that the titer may be increased and the reactions occur at high temperature. This consideration alone illustrates the difficulty in formulating rigid criteria for any classification.

The Kell-Cellano Factors (Kk). Genetically, these two factors (8 and 99.8 per cent positive reactions) are alleles with equal dominance, as in the MN or Rh-Hr system.[5] Clinically, the Kell factor is far more important in hemolytic disease and transfusion reactions in contrast to the Cellano factor and actually only one sample of

anti-Cellano has been reported in a mother of a mildly affected infant.* Race and Sanger[6] list nine cases of anti-Kell as the only antibody in a serum, seven with hemolytic disease and two with transfusion reactions. As is to be expected, Cellano negative individuals (*KK*), 0.2 per cent in the general population, are found more frequently in the offspring of double heterozygous matings.

The Duffy Factor (*Fy*^{a}).[7] Five samples of this antibody are listed by Race[6] and, in addition, two others have been studied by Levine, Kellner, and Haber[8] and Brown,[9] respectively. In most instances, the antibody occurred along with others, but there is no doubt that this factor (65 per cent positive reactions) is important in transfusions. At present, there is no evidence that this factor plays a role in hemolytic disease. With the exception of Brown's case,[9] anti-Duffy can be detected only with the aid of the indirect Coombs test and it has been suggested by Kabat[10] that failure to elicit reactions with trypsinized or albumin-suspended cells may be due to very few antigenic 'molecules' on the surface of the red cell. In the case of Rosenfield, Vogel, and Race,[11] the pre-transfusion specimen had a weak anti-Duffy, so that the first transfusion resulted in a hemolytic reaction. Since this patient had a benign prostatic tumor, the possibility is not excluded that the immunization was brought about by tumor cells.

The MN System. By and large, human anti-M and anti-N sera are not important in transfusion because the reactions which occur at low room temperature disappear at 37°C. It is assumed that these are naturally occurring antibodies which on antigenic stimulation may increase in titer and become active also at 37°C. and, under these conditions, may cause hemolytic transfusion reactions, as in the cases of Broman[12] (anti-M) and Singer[13] (anti-N).

The S factor (54 per cent positive reactions) was shown to be genetically related to the MN factor, either as a third allele or as a closely linked gene.[14, 15]* Anti-S has been found as the only antibody in two cases (Vogel and Rosenfield,[16] Coombs[17]), and, in both cases, the antibody behaved like an atypical agglutinin inactive at 37°C. More frequently, anti-S was found along with anti-D produced by previous transfusions or associated with hemolytic disease. In at least one case (Mollison), anti-S could be demonstrated with the aid of the indirect Coombs test. Apparently, anti-S, like anti-Duffy, when it occurs by itself as an immune antibody, may cause hemolytic transfusion reactions, but not hemolytic disease, at least on the basis of about eight examples of anti-S.

The Lewis System (*Le*^{a} *and Le*^{b}). Shortly after Mourant[2] described anti-Le^a, numerous examples of this naturally occurring antibody were observed. Although not important clinically, anti-Le^a is remarkable because it reacts almost exclusively with the blood of non-secretors of groups O, A, and B.[18] The allelic factor, Le^b, was found by Andresen[19] as a naturally occurring agglutinin which gave about 75 per cent reactions. These blood factors are unusual because of their unique genetic behavior, positive reactions indicating the non-secretor which is recessive to the secretor status.

The Lutheran Factor Lu^a). Anti-Lu^a was observed in sera of transfused patients so that the possibility is not excluded that they may still be naturally occurring antibodies.[20] They are probably not important in transfusions, since only weak transient antibodies were produced in experimental transfusions of Manwaring and Pickles.[21] Lu^a is inherited as a Mendelian dominant.

The Kidd Factor (*Jk*^{a}). Anti-Jk^a was found by Allen and Diamond[22] in a serum of a mother which contains also anti-Kell. Initially a saline agglutinin, the antibody on storage had a titer of 1:64 by the Coombs method and it gave 77 per cent positive reactions.

* *Cf. Addendum.*

"Private" Blood Factors. These blood factors are characterized either by a remarkably very high or equally low incidence in the general population, the positive or negative reactions being limited to members of a particular family. In each case, only one sample of the antibody has been observed, so that the individuality of the factors or their mutual independence is still to be established by comparative tests with the rare bloods either lacking or containing the factor. The essential facts in each of the five blood factors and their antibodies are listed in TABLE 2.

Table 2
"Private" Blood Factors

*Name**	+	−	*Nature of antibody*	*Antigenic stimulus*	*Other antibodies in serum*
1. Levay[20]	3/7†	350	intermediate? immune?	transfusion	c, C^w, N, Lu^a
2. Gr.[23]	5/8†	191	naturally occurring	none	none
3. Jobbins[24]	2/4†	120	immune, indirect Coombs	pregnancy	anti-D
4. Miltenberger[25]	4/9†	320	immune, indirect Coombs	pregnancy	none
5. Jay[26]	1500	2/8†	immune, hemolysin	gastric tumor	none

* See bibliography, as indicated by reference numbers, for sources.
† The numerator indicates the number of individuals in one family whose blood contains or lacks the factor. The denominator denotes the total number in the family whose blood was tested. In the Jay case, the factor was absent in the blood of the propositus and her sister only (*cf. Addendum*).

Table 3
Old 'New' Blood Factors*

Author		+	−	
Ottenberg & Johnson[27]		40	60	naturally occurring, dominant
Landsteiner & Levine[28]		40	60	intermediate, stimulated by transfusion
Neter[29]		25	75	hemolysin, stimulated by transfusion
Levine & Polayes[30]		26	74	hemolysin, stimulated by pregnancy?
Landsteiner, Strutton, & Chase[31]	(colored)	7.3	92.7	immune heteroagglutinin (rabbit)
	(white)	0.5	99.5	

* This list is necessarily incomplete.

The Jay Factor (Tj^a)[26]. This blood factor is of interest because the tumor cells specifically absorbed the antibody. As a preliminary explanation for the presence in the tumor of a genetically determined factor not present in the red cells, one may suggest a genetic mutation which at the same time may have induced the tumor itself.

Finally, mention may be made of several blood factors observed many years ago but more recently neglected. Aside from one case, these factors were not identified by names. Nothing is known about the mutual independence of two sets of antibodies which give almost identical incidence of reactions. Remarkably enough, two of the antibodies are hemolysins. The essential facts in these five cases are listed in TABLE 3.

Space does not permit a discussion of the difficulties encountered in transfusing patients with antibodies reacting on the vast majority of individuals, such as anti-e, anti-k or anti-Tj^a. When a serum is shown to contain numerous antibodies, it is important to identify the antibodies and to select bloods compatible with the warm immune antibodies. Other antibodies in the serum may not be significant in selection of donors. In other rare cases, as in the Buxton blood, where there is a deletion of part of the chromosome, the simultaneous presence of anti-C and anti-c makes it impossible to select compatible blood, unless it be the very rare blood characterized by the identical chromosomal anomaly.[6]

In general, recent observations make it possible to render decisions of non-paternity in an ever increasing number of cases, provided that carefully selected potent antibodies become available.

With the exception of the Lewis factors, Le^a and Le^b, which show a relationship to the secretor status of the ABO system, all blood factors studied were shown to be independent of each other. To all intents and purposes, the concept of

individuality of human blood is now fully established, and it is not surprising that the method employed, iso-immunization, was the same one used for the first time in 1900 by Ehrlich and Morgenroth for demonstrating the individuality of goat blood and later successfully extended by other workers to several other species of animals.

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Addendum. Very recently, Levine and his co-workers described another blood factor identified as "s," an allelic to S. Anti-s, produced by transplacental isoimmunization, was the only atypical antibody in the serum of a mother whose infant suffered from hemolytic disease. A second example of anti-Cellano was also observed in an Rh negative patient whose serum also contained anti-D, the antigenic source for which was derived from transfusions. A second example of anti-Tj[a] was identified in a serum of a woman in South Africa, probably immunized by transfusions (Levine and Zoutendyk).

* Limits imposed on these papers make it necessary to reduce the number of references to a minimum. A more complete list of papers is given by Race and Sanger.[6]

ERYTHROCYTE-ISOANTIBODY REACTIONS IN DOGS*

By L. E. YOUNG, W. A. O'BRIEN, G. MILLER, S. N. SWISHER, D. M. ERVIN, R. M. CHRISTIAN, AND C. L. YUILE†

The purpose of this paper is to outline briefly the scope of our recent observations on the specificity and *in vitro* behavior of dog iso-antibodies, on hemolytic transfusion reactions, and on hemolytic disease in new-born dogs. Analogies with human erythrocyte-antibody reactions and with mechanisms of destruction of human red cells have been apparent at every turn in the course of these studies.

Specificity of Dog Isoantibodies. Of the many antigenic factors that may be present in dog erythrocytes, the six thus far studied most extensively in our laboratory[1, 2] are designated canine A, B, C, D, E, and F in the order of their demonstration. The letters assigned carry no implication of any relationship between the canine factors and the human blood groups designated by the same letters. The proportions of random dogs having cells that react with anti-A, -B, -C, -D, and -E are shown in TABLE 1. Since the only available anti-F serum is not of sufficient potency to be used in extensive testing, results obtained thus far with this serum are not tabulated. Preliminary tests suggest, however, that anti-F may identify a dog red cell factor

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TABLE 1

FREQUENCY OF REACTION OF ERYTHROCYTES FROM RANDOM DOGS WITH 5 TYPES OF DOG ANTISERA

Serum	*Number of dogs tested*	*Number of dogs positive*	*% positive*
Anti-A	460	290*	63
Anti-B	460	28	6
Anti-C	460	454	99
Anti-D	321	87	27
Anti-E	138	110	80

* This figure includes both A and A′ dogs. A′ cells are defined as those which, when tested against a battery of anti-A sera, are not directly agglutinated by all of the antisera but are regularly sensitized for the antiglobulin reaction by nearly all anti-A sera.

TABLE 2

FREQUENCY OF COMBINATIONS OF AGGLUTINOGENS IN 138 RANDOM DOGS

Agglutinogens	*Number of dogs*	*Agglutinogens*	*Number of dogs*
ACE	52	ABCE	4
CE	23	AE	2
ACDE	13	BCE	2
AC	10	ABC	1
C	10	ABE	1
ABCDE	8	ACD	1
CDE	5	(F)	1
CD	5		

during the period of immunization. Anti-A developed in substantial titers within 40 days in most of the A-negative dogs transfused with A-positive cells. Anti-A appeared in 9 to 14 days in three dogs after a single transfusion. Limited experience with the production of other dog iso-antibodies has shown that in general they develop somewhat more slowly than anti-A. Anti-D occurs naturally in very low titers in some D-negative dogs, while the other iso-antibodies have been found only after transfusion.

The data on the frequency with which A, B, C, D, and E factors are found in the red cells of random dogs can be used to compute the potentiality for production of the respective iso-antibodies in dogs transfused indiscriminately, as indicated in TABLE 4. For example, 37 per cent of random dogs lack the canine A factor, while 63 per cent of random donors for such dogs have red cells containing the A factor. Since $0.37 \times 0.63 = 0.23$, it can be stated

TABLE 3

Agglutinogen present in transfused red cells and lacking in recipients' red cells	*Number of dogs transfused*	*Number of dogs developing iso-antibody*	*Number of dogs developing iso-antibody in given number of days after first immunizing transfusion.* Days 8–14	15–40	41–100	101–206
A	23	21*	8	10	1	2
B	5	4		2	1	1
C	2	2	1	1		
D	5	2	2†			
E	3	2	1			1

* The 2 dogs that failed to develop anti-A after multiple transfusions of A-positive cells were thought to be A-negative. In retrospect, however, it is considered possible that these dogs had A′ cells and were therefore incorrectly typed by the methods in use at the time. Both of these dogs died before newer methods of typing red cells were in use.

† One of the D-negative dogs that ultimately developed anti-D in high titer had a small amount of anti-D in the serum prior to transfusion.

allelic to the canine C agglutinogen. TABLE 2 gives the distribution of various combinations of A, B, C, D, and E among the first 138 random dogs tested with a large battery of dog antisera.

Antigenicity of Agglutinogens in Dog Erythrocytes. Data on development of iso-antibodies in transfused dogs are summarized in TABLE 3. In most instances, 5 to 20 cc. of whole citrated blood were injected intravenously once or twice weekly

that approximately 23 per cent of random transfusions have the potentiality for production of anti-A in the recipients.

Since all dogs lacking an agglutinogen cannot be expected to develop the corresponding iso-antibodies after transfusion, further experience will be necessary before reliable estimates can be made regarding the proportion of dogs that can be expected to produce iso-antibodies against any given canine agglutinogen. Experience already

summarized indicates, however, that perhaps 40 per cent of A-negative dogs will develop anti-A within 14 days after transfusion of A-positive cells and that 80 to 90 per cent may develop anti-A within 40 days if one or two small transfusions are given weekly. Since anti-A is a potent *in vivo* hemolysin, these estimates have obvious significance to veterinarians and to the numerous investigators engaged in transfusing dogs for one purpose or another.

TABLE 4

ESTIMATES OF FREQUENCY OF POTENTIALLY IMMUNIZING TRANSFUSIONS IN DOGS TRANSFUSED INDISCRIMINATELY WITHOUT RESPECT TO BLOOD TYPE

Agglutinogen	*% random dogs having agglutinogen in red cells*	*% random dogs lacking agglutinogen*	*% random transfusions potentially immunizing*
A	63	37	23
B	6	94	6
C	99	1	1
D	27	73	20
E	80	20	16

Anti-B, -C, and -D, on the other hand, appear to be of less importance so far as *in vivo* destruction of dog red cells is concerned. Moreover, these antibodies are in general produced more slowly than anti-A and it is apparent from TABLE 4 that the potentiality is relatively low for production of anti-B and -C in dogs transfused at random.

In Vitro *Behavior of Dog Iso-antibodies.*[1, 2] Observations on the *in vitro* behavior of dog iso-antibodies are summarized in TABLE 5. Anti-A acts as a hemolysin as well as an agglutinin and is most active at 37°C. It fixes complement and readily sensitizes red cells for the antiglobulin reaction. The ability of anti-A to agglutinate dog cells is enhanced by a heat labile component of normal dog serum, which is quite likely a specific fraction of dog complement. Activity of anti-A is irregularly augmented by bovine and human albumin, but not by fresh guinea pig serum.

Anti-B, -C, and -D are hemagglutinins having wide thermal amplitude and apparently maximal activity in saline media. They do not readily sensitize red cells for the antiglobulin reaction, but sensitization has occasionally been produced by anti-C and anti-D.

Anti-E is most active at low temperatures but also agglutinates E-positive cells at 37°C. It does not hemolyze cells or fix complement, but its activity is enhanced by a component of normal dog serum and it readily sensitizes cells for the antiglobulin reaction.

TABLE 5

SUMMARY OF CHARACTERISTICS OF DOG ISO-ANTIBODIES

Characteristics	*Anti-A*	*Anti-B, -C, and -D*	*Anti-E*
Thermal amplitude	most active at 37 C.	equally active at 4 C. to 37 C.	most active at 4 C.
Heat stability	thermostable	thermostable	thermostable
Hemolytic activity	marked	none	none
Complement fixation	marked	none	none
Enhancement of agglutination by a heat labile component of normal dog serum	marked	none	moderate
Sensitization of cells for the antiglobulin test	regular	usually none; occasionally with anti-C and anti-D	regular

Attachment of anti-A in some dog sera to the A factor of some dog red cells is demonstrable only by use of the Coombs antiglobulin reaction. Dog cells reacting in this way are currently called A′. If selected undiluted anti-A sera are tested with A-positive erythrocytes, various degrees of reactivity become apparent. All A′ cells that fail to agglutinate directly when mixed with anti-A sera are strongly agglutinated after being washed and mixed with antiglobulin serum. There is apparent similarity, therefore, of the A′

series and the Rh_0 or D^u variants described by Wiener,[3] Stratton,[4] and Race, Sanger, and Lawler.[5] Inheritance of A′ has been studied in two litters involving five whelps and in each instance the order of reactivity of the red cells with anti-A sera was the same as in the parent.

Transfusion of Incompatible Red Cells. Hematologic, serologic, and renal alterations produced in dogs by transfusion of incompatible red cells have been described in earlier reports from this laboratory.[6, 7] It might be re-emphasized here, however, that dogs withstand incompatible transfusions remarkable well. In more recent experiments,[8] two successive transfusions of A cells were given at intervals of two hours to immunized A-negative recipients. In each experiment, the donated cells tagged with radioactive iron were destroyed more slowly after the second transfusion than after the first, and hemoglobinemia produced by the first transfusion was not appreciably augmented by the second. The successive transfusions at short intervals were considered nearly equivalent to single large transfusions but permitted separate study of the effects produced. In the presence of adequate complement, the rate of hemolysis appeared to be limited by the disappearance of antibody when the initial titer of antibody was low. In dogs with relatively high antibody titer, the rate of disappearance of incompatible cells was limited to some extent by available complement.

When dog cells containing the B factor were transfused to a recipient previously immunized against this factor, there was no subsequent hemoglobinemia or bilirubinemia, but small clumps of cells could be detected in samples of the recipient's blood for about two days. Studies on the survival of B, C, D, and E cells, after transfusion into previously immunized recipients, are being made with the Ashby technique.

Differential Agglutination of Dog Erythrocytes. A technique of the Ashby type has been developed for differential agglutination of D-positive dog red cells by potent anti-D serum. By use of this technique, normal D-negative dog red cells were found to disappear in a linear manner over a period of more than 100 days after transfusion into normal D-positive recipients.[9] The method of selective agglutination of dog red cells is currently being used to study the survival of D-negative cells after transfusion into D-positive recipients under a variety of conditions. These studies have widened considerably the scope of our program dealing with canine immunohematology.

Transfusions of Incompatible Plasma.[2] When plasma containing anti-A was transfused, even in relatively small amounts, into A-positive dogs with renal bile fistulae, the recipients developed hemoglobinemia, hyperbilirubinemia, anemia, and spherocytosis. The recipients' red cells became more susceptible to lysis in hypotonic saline and were readily agglutinated by antiglobulin serum for periods as long as 23 days. Bilirubin excretion through the renal-bile fistulae was substantially increased during the first week after transfusion. Alterations in the red cells and in pigment excretion produced by the dog iso-antibody might be compared with those produced by transfusions of human plasma from so-called "dangerous universal donors" and by human auto-antibody in active cases of acquired hemolytic anemia. When anti-A plasma was transfused to A′ recipients, on the other hand, there was much less hemolytic effect than in the case of the so-called "strong A" recipients. In these experiments the A′ cells were strongly sensitized for the antiglobulin reaction but the rate of red cell destruction was not rapid. Findings in the A′ recipients were comparable to those encountered in quiescent human cases of acquired hemolytic disease.

Plasma containing anti-B, -C, -D, and -E agglutinins appeared to have remarkably little hemolytic effect when given in large large amounts to dogs having cells contain-

ing the corresponding antigens. After none of these transfusions was it possible to demonstrate hemoglobinemia, bilirubinemia, sphering, or increased osmotic or mechanical fragility of the recipient's cells. In some of the experiments, however, samples of the recipient's blood contained small clumps of agglutinated red cells. In five of eight experiments with anti-C plasma, the recipient's C-positive cells were rendered unagglutinable by anti-C for periods of 7 to 28 days; that is, they exhibited the blocking phenomenon.

Observations on Hemolytic Disease in New-born Dogs.[18] In A-negative bitches immunized by intravenous injections of A-positive cells and mated with A-positive sires, the anti-A titers regularly declined during pregnancy unless artificially stimulated by an additional transfusion of A cells near term. Anti-A titers in the colostrum exceeded those in the maternal serum and all A-positive pups born to such bitches developed hemolytic disease provided they suckled the immunized bitch during the first day of life. Anemia and increased osmotic fragility of the red cells of the A-positive pups usually became apparent within the first 16 hours after birth.

Pups acquired little or no anti-A if fed cow's milk during the first 16 to 24 hours of of life before suckling an immunized bitch. Pups born to a non-immunized bitch, on the other hand, developed hemolytic disease when allowed to suckle an immunized bitch during the first few hours of life. No iso-antibody has been demonstrable in pups at birth and no gamma globulin could be detected by electrophoretic study of pooled plasma from two litters of new-born pups. Pups born to bitches immunized against the canine C or E factors failed to develop hemolytic disease even though they suckled breast milk containing anti-C or anti-E soon after birth.

Conclusion. From the studies briefly cited it is apparent that many of the *in vitro* and *in vivo* reactions between dog red cells and iso-antibodies are analogous to those observed in human blood. Numerous problems in the field of immuno-hematology can be investigated to advantage by utilizing the dog antigen-iso-antibody systems already identified.

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THE IDENTIFICATION OF HEMAGGLUTININS WITH RED CELLS ALTERED WITH TRYPSIN

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The studies of Hübener[1] and Thomsen[2] first showed that enzymes present in certain bacterial filtrates could activate latent red cell agglutinogens. This observation was later popularized by Friendenreich[3] as "T agglutination."

Pickles,[4] while attempting to compare "T agglutination" and the auto-sensitization of acquired hemolytic anemia, found that the broth filtrate of Vibrio cholera, besides activating "T agglutinogen," could make red cells agglutinable in saline to specific incomplete Rh antibodies. The exact enzyme in cholera-filtrate responsible for alteration of the Rh agglutinogen has not been identified, but Morton and Pickles[5]

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found that the same alteration of the Rh factor could be produced by the enzyme, trypsin.

Wheeler, Scholl, and Luhby[6] found that a simple 0.1 per cent suspension in saline of Difco Trypsin 1:250 could change red cells so that they could be specifically agglutinated in saline by incomplete Rh antibodies to a degree at least as good as that obtained with albumin. Wheeler, Luhby, and Scholl[7] later published a more extensive report in which they stated that a variety of proteolytic enzymes were active in this manner: chymotrypsin, bromelin, and Russell viper venom in their hands, and pepsin, papain, and erepsin in independent studies of Quilligan.[8]

Recently, Wiener and Katz[9] reported activity for trypsin, papain, chymotrypsin and ficin.

Technic

Although 0.1 per cent Difco Trypsin 1:250 in saline alters red cells so that they are readily agglutinable by specific incomplete Rh antibodies, in our hands this technic is neither qualitatively constant nor quantitatively maximal. Along lines first suggested by Morton and Pickles,[5] we have prepared a standard stable solution of trypsin as follows: A 10 per cent suspension of Difco Trypsin 1:250 in N/20 HCl is aged at 4°–10°C. for 7–10 days. Trypsin is inactive but stable and soluble at this concentration of acid. In addition to extraction of the enzyme, there is an intense isoelectric (pH 4.5–4.8) flocculation of crystals of trypsin-trypsin inhibitor compound.[10] After extraction, the supernatant fluid is passed through a Seitz pad to remove yeasts and molds and to obtain a clear amber filtrate that is stable for 2–6 months at 4°–10°C. Occasionally, there is further flocculation of trypsin-trypsin inhibitor crystals in the filtrate, but the potency of the acid trypsin solution is not noticeably affected. For use, the acid trypsin solution is diluted 1:10 with Sorenson phosphate buffer of pH 7.2–7.4, and further diluted 1:3 with saline. This working solution is not stable for more than one day.

Trypsin treatment of red cells is accomplished as follows: A 2–4 per cent saline suspension of red cells is prepared in the usual fashion. Cells from clotted blood are washed once to remove serum trypsin-inhibitor; cells from uncoagulated blood are washed three times to remove all traces of fibrinogen, which is clotted by proteolytic enzymes. The cell suspension is centrifuged and the supernatant saline removed and replaced with an equal volume of working trypsin solution. After mixing, the cells in trypsin are incubated at 37°C. for 15 minutes. The treatment is then stopped by centrifuging, removing the supernatant trypsin solution, and resuspending the red cells in fresh saline. Suspensions of treated red cells are stable at cool room temperature for several hours and at 4°–10°C. for 12–24 hours. They are used in the same manner as ordinary red cell suspensions.

Optimal activity of trypsin on red cells occurs over a considerable span of pH. Although the trypsin effect is inhibited at pH 6.0 and below, it is intact from pH 6.4–8.5 (TABLE 1). Buffered dilutions of trypsin between 1:20 and 1:100 should render red cells more sensitive to weak concentrations of Rh antibody than trypsin at other strengths, and such treated cells should not give reactions with normal serum devoid of specific iso-antibodies. The peak activity of the trypsin solution is generally 1:30 (TABLE 2).

The enzyme papain similarly alters red cell agglutinogens. Papain, Merck (N. F., 1:200), can be stabilized in an acid solution just as Difco Trypsin 1:250. The 10 per cent suspension in N/20 HCl is extracted for only two days at 4°–10°C. The filtered acid solution is not as stable as trypsin and frequently causes slight hemolysis of red cells. Otherwise, the activity is identical with that of trypsin. Standardization of acid papain solution is also by cross-titration. The working dilution is generally 1:30, buffered with Sorenson phosphate buffer pH 7.2–7.4, the same as for trypsin.

Chymotrypsin alters red cells just as trypsin does. We have not studied erepsin, bromelin, or ficin. We have been unable to affect the Rh agglutinogen with trypsinogen, chymotrypsinogen, pepsin, Russell viper venom, or thromboplastin.

The agglutination of enzyme-treated red cells with iso-antibodies is clear-cut. The agglutinates show little or no tendency to adhere electrostatically to glass, as is frequently observed with other technics. Centrifugation is, therefore, unnecessary.

TABLE 1

EFFECT OF pH ON THE ACTIVITY OF 0.1 PER CENT DIFCO TRYPSIN 1:250

	Range of pH of trypsin treatment								
	4.50	*4.98*	*5.50*	*6.01*	*6.40*	*7.00*	*7.75*	*8.00*	*8.50*
Strength of Reaction with with Incomplete Rh Antibodies (Mrs. So) Titer 1:800	++	++±	++±	+++	+++±	+++±	+++±	+++±	+++±

Four per cent washed type O Rh_1Rh_2 red cells suspended in freshly prepared phosphate (Sorenson) buffered saline solutions of varying pH, each containing 0.1 per cent Difco Trypsin 1:250, and incubated 10 min. at 37°C. Cells then washed in saline once and reconstituted to 4 per cent suspension in fresh saline. Cell suspension (0.1 ml.) and incomplete Rh serum (0.1 ml.) incubated 1 hr. at 39°C.

TABLE 2

STANDARDIZATION BY CROSS-TITRATION OF ACID TRYPSIN SOLUTION (DIFCO TRYPSIN 1:250)

Dilutions of incomplete Rh serum, Mrs. So	*Buffered dilutions of acid trypsin* 2	4	8	16	32	64	128	256	512	1024	*Indirect anti-globulin test*
1:1	+++	+++	++++	++++	++++	+++	+	−	−	−	++++
1:10	+±	+++	+++	++++	++++	+++	++	+	−	−	++++
1:100	±	++	+++	++++	++++	++++	+++	+	−	−	++++
1:1000	−	±	++	+++	+++	+++	+++	++	+	−	++++
1:4000	−	−	+	+	+±	+	+	±	−	−	++±
1:16,000	−	−	−	−	±	−	−	−	−	−	++
1:64,000	−	−	−	−	−	−	−	−	−	−	±
Saline	−	−	−	−	−	−	−	−	−	−	−
Normal serum	+	+	±	−	−	−	−	−	−	−	−

Acid trypsin solution: 10 per cent Difco trypsin 1:250 extracted in N/20 HCl at 4°-10°C. for 7 days and then filtered through a Seitz pad.

Dilutions of acid trypsin made with phosphate (Sorenson) buffered saline pH 7.2

Type O Rh_1Rh_2 red cells suspended in 4 per cent concentration in various strengths of trypsin for 15 min. at 37°C., then washed once and resuspended in fresh saline.

Cells for anti-globulin test not treated with trypsin.

Results

The Rh-Hr Factors. Enzyme treatment alters all of the known Rh-Hr factors similarly. Treated cells are agglutinable in saline by specific incomplete Rh-Hr antibodies. The factors studied have included Rh_0, rh′, rh″, hr′, hr″, and rh^w. We confirm the observation of Wheeler, *et al.*,[7] that the prozone phenomenon obtained with certain high-titer sera in albumin is not observed with optimally trypsin-treated red cells.

Enzyme-treated red cells are sensitive to agglutination by extremely small amounts of Rh antibody. We have not observed an instance of sensitization to Rh-Hr that has not been detectable by this technic, and Wiener and Katz[9] have reported a similar observation. This fact makes the enzyme technic just as valuable in routine tests for Rh sensitization as the indirect antiglobulin test. TABLE 3 illustrates some typical Rh antibody titrations, to show the variation in avidity and end-point with the methods now used to demonstrate these antibodies.

Enzyme treatment does not change significantly the quantity of Rh substance in the red cell. Adsorption and elution of Rh antibodies with enzyme-treated cells do not differ markedly from the result obtained with unaltered cells (TABLE 4). This result is at variance with observations of Wheeler *et al.*[7]

TABLE 3

(A)

THE BEHAVIOR OF ANTI-Rh SALINE AGGLUTININS AGAINST O Rh_1Rh_2 RED CELLS. A COMPARISON OF SALINE, ALBUMIN, TRYPSIN, AND ANTI-GLOBULIN TECHNIQUES

Serum	*Titer*	*Method*	*Dilutions*									
			10	*20*	*40*	*80*	*160*	*320*	*640*	*1280*	*2560*	*5120*
	480	Sal.	++++	+++±	++±	+±	+	±	tr	−	−	−
Mrs. Fe.	480	Alb.	++++	++++	+++±	++	+	±	tr	−	−	−
10/17/49	480	Tryp.	++++	++++	+++	++	+	±	tr	−	−	−
	480	A-Gl.	++++	++++	+++±	+++	+±	+	tr	−	−	−
			10	*20*	*40*	*80*	*160*	*320*	*640*	*1280*	*2560*	*5120*
	80	Sal.	++±	++	+	±	−	−	−	−	−	−
Mrs. Sc.	80	Alb.	+++	++	+	±	−	−	−	−	−	−
10/14/49	160	Tryp.	++++	+++±	+++	+±	±	−	−	−	−	−
	160	A-Gl.	++++	+++±	+++	++	+	−	−	−	−	−
			1	*2*	*4*	*8*	*16*	*32*	*64*	*128*	*256*	*512*
	48	Sal.	+++	+++	++	+±	+	±	tr	−	−	−
Mrs. Ma.	48	Alb.	+++	+++	++	++	+	±	tr	−	−	−
10/26/49	192	Tryp.	+++±	+++±	+++±	+++	+++	++	+	±	tr	−
	8	A-Gl.	++	++	+	±	−	−	−	−	−	−

(B)

THE BEHAVIOR OF ANTI-Rh INCOMPLETE ANTIBODIES AGAINST O Rh_1Rh_2 RED CELLS. A COMPARISON OF SALINE, TRYPSIN, ALBUMIN, AND ANTI-GLOBULIN TECHNIQUES

Serum	*Titer*	*Method*	*Dilutions*									
			1	*10*	*20*	*40*	*80*	*160*	*320*	*640*	*1280*	*2560*
	<1	Sal.	tr	−	−	−	−	−	−	−	−	−
Mrs. Ge.	160	Alb.	++++	+++	++±	++	+	±	−	−	−	−
10/21/49	160	Tryp	++++	+++	++	++	+	±	−	−	−	−
	240	A-Gl.	++++	++++	++++	+++	++	±	tr	−	−	−
			1	*2*	*4*	*8*	*16*	*32*	*64*	*128*	*256*	*512*
	0	Sal.	−	−	−	−	−	−	−	−	−	−
Mrs. Le.	16	Alb.	++	++	+±	+	±	−	−	−	−	−
11/18/49	64	Tryp.	++++	++++	++++	++++	+++±	++	±	−	−	−
	512	A-Gl.	++++	++++	++++	++++	+++	++±	++±	++±	+	±
			1	*10*	*20*	*40*	*80*	*160*	*320*	*640*		
	0	Sal.	−	−	−	−	−	−	−	−		
Mrs. Sc.	30	Alb.	++	+	±	tr	−	−	−	−		
10/24/49	160	Tryp.	++++	++++	++++	+++	+±	+	−	−		
	40	A-Gl.	+++	+++	++	±	−	−	−	−		

(C)

THE BEHAVIOR OF ANTI-Rh ANTIBODIES IN UMBILICAL VEIN SERA OF NEWBORNS SUFFERING FROM *Erythroblastosis Fetalis*: A COMPARISON OF SALINE, ALBUMIN, TRYPSIN, AND ANTI-GLOBULIN TECHNIQUES (O Rh_1Rh_2 RED CELLS)

Serum	*Titer*	*Method*	*Dilutions*									
			1	*2*	*4*	*8*	*16*	*32*	*64*	*128*	*256*	*512*
	0	Sal.	−	−	−	−	−	−	−	−	−	−
Baby Sc.	1	Alb.	±	−	−	−	−	−	−	−	−	−
10/6/49	12	Tryp.	+++	+++	++	+±	tr	−	−	−	−	−
	16	A-Gl.	+++±	+++	++	++	±	−	−	−	−	−
	0	Sal.	−	−	−	−	−	−	−	−	−	−
Baby Mc.	<1	Alb.	tr	−	−	−	−	−	−	−	−	−
10/4/49	8	Tryp.	++++	+++±	+±	±	−	−	−	−	−	−
	8	A-Gl.	++±	++	+	±	−	−	−	−	−	−
	0	Sal.	−	−	−	−	−	−	−	−	−	−
Baby Ge.	1	Alb.	+	−	−	−	−	−	−	−	−	−
10/28/49	1	Tryp.	++	−	−	−	−	−	−	−	−	−
	6	A-Gl.	++	+	±	tr	−	−	−	−	−	−

TABLE 3—*Continued*

(D)

THE BEHAVIOR OF ANTI-Rh ANTIBODIES AGAINST O Rh_1Rh_2 RED CELLS: A COMPARISON BETWEEN MATERNAL AND NEWBORN (RHRH) SERA BY THE SALINE, TRYPSIN, AND ANTI-GLOBULIN TECHNIQUES

Serum	*Titer*	*Method*	*Dilutions* 1	2	4	8	16	32	64	128	256	512
	256	Sal.	++±	+++	+++±	+++	+++	++±	+±	±	±	–
Mrs. Be.	384	Tryp.	++++	++++	++++	++++	+++±	+++±	+++	+	±	tr
12/28/49	48	$A\text{-}Gl_a$	+++±	+++±	+++	++±	++	±	tr	–	–	–
	12	$A\text{-}Gl_b$	+++	+++	+	±	tr	–	–	–	–	–
	0	Sal.	–	–	–	–	–	–	–	–	–	–
Baby Be.	32	Tryp.	++++	++++	++++	++++	+++	++	–	–	–	–
12/28/49	64	$A\text{-}Gl_a$	+++±	+++	++±	++	+±	+	±	–	–	–
	3	$A\text{-}Gl_b$	+++	++	tr	tr	–	–	–	–	–	–

Note: $A\text{-}Gl_a$—immediate centrifugation and reading. $A\text{-}Gl_b$—reading after one hour at 20°C.

TABLE 4

ABSORPTION AND ELUTION OF Rh ANTIBODIES. EFFECT OF TRYPSIN TREATMENT ON THE QUANTITY OF Rh ANTIGEN TITRATIONS IN SALINE AGAINST TRYPSIN-TREATED TYPE O Rh_1Rh_2 RED CELLS

	10	*20*	*40*	*80*	*160*	*320*	*640*	*1280*	*2560*	*Titer*
Native serum, Es	++++	++++	++++	++++	+++±	+++	++±	++	+	2560
After 1 absorption with trypsin treated O Rh_1Rh_2 cells	++++	++++	++	+	±	tr	–	–	–	160
After 1 absorption with unaltered O Rh_1Rh_2 cells	++++	++++	+++	+	±	tr	–	–	–	160

	0	*2*	*4*	*8*	*16*	*32*	*64*	*128*	*256*	*Titer*
Eluate from trypsin-treated absorbing cells	+++	++±	++	+	±	tr	–	–	–	16
Eluate from unaltered absorbing cells	++	++	+±	+	±	tr	–	–	–	16

Serum absorbed with an equal volume of packed red cells. Trypsin treatment 15 min. at 37°C. in 4 per cent cell suspension; cells subsequently washed once with saline and packed for absorption. Packed cells after absorption washed with 10 volumes of saline 8×; Rh antibody eluted into ½ volume of saline at 56°C. for 15 min.

The A-B-O Factors. Enzyme treatment alters the A and B factors so that agglutination is of better avidity, and titration values are higher than in saline. The results approximate, but are not identical with, those obtained with the indirect antiglobulin technic and the acacia technic.[11]

The O factor is not essentially changed by enzyme treatment as far as can be seen with either eel serum or human serum as the source of anti-O.

The M-N Factors. The M-N factors are largely destroyed by the enzyme treatment of red cells. When trypsin-treated red cells are used to absorb reagent anti-M (rabbit or human) or anti-N (rabbit) sera, the M or N antibodies are not removed. Human anti-M serum fails to agglutinate trypsin-treated M positive red cells. Rabbit anti-M and N sera agglutinate enzyme-treated red cells, but the agglutination is for the most part non-specific and similar to "T agglutination" (TABLE 5). Wiener and Katz[9] suggested that the panagglutination of trypsin-treated red cells by rabbit anti-M and anti-N sera was dependent upon the presence of unabsorbed incomplete antibody. Guinea pig anti-rabbit globulin serum demonstrates only small amounts of such incomplete antibody, however, and "T-like agglutination" would appear to be a better explanation.

The S Factor. The S factor is enhanced by enzyme treatment so that agglutination of red cells with our only example of anti-S serum is improved from ++ readings of unaltered S positive cells to +++± and ++++ readings of the same cells treated

with trypsin. This result differs markedly from that obtained with the genetically closely linked M-N factors. The observation is at variance with the cited findings of Pickles.[12]

very little Fy^a antibody and are unagglutinated in the indirect antiglobulin test (TABLE 6).

TABLE 5

THE EFFECT OF TRYPSIN TREATMENT ON THE M ANTIGEN

	Unaltered RBC			*Trypsin-treated RBC*		
	M	*MN*	*N*	*M*	*MN*	*N*
Human anti-M serum	++++	+++±	−	−	−	−
absorbed with M cells, unaltered	±	−	−	−	−	−
absorbed with M cells, trypsin-treated	++++	+++±	−	−	−	−
absorbed with N cells, unaltered	++++	+++±	−	−	−	−
absorbed with N cells, trypsin-treated	++++	+++±	−	−	−	−
Reagent rabbit anti-M serum	++++	++++	−	++±	++±	++±
absorbed with M cells, unaltered	−	−	−	−	−	−
absorbed with M cells, trypsin-treated	++++	++++	−	−	−	−
absorbed with N cells, unaltered	++++	++++	−	++±	++±	−
absorbed with N cells, trypsin-treated	++++	++++	−	−	−	−

Serum absorbed with an equal volume of packed red cells. Trypsin treatment 15 min. at 37°C. in 4 per cent cell suspension; cells subsequently washed once with saline and packed for absorption. Agglutination reactions: cell suspension (0.1 ml.) with anti-M serum (0.1 ml.) room temp. for 1 hr.

TABLE 6

THE EFFECT OF TRYPSIN TREATMENT ON THE Fy^a ANTIGEN

A. *Primary reactions of anti-*Fy^a *serum and* Fy^a *positive red cells*

Technic	*Trypsin-treated RBC*	*Unaltered RBC*
Saline	−	−
Albumin	±	−
Indirect antiglobulin	−	+++

B. *Absorption of* Fy^a *antibodies*

	0	*2*	*4*	*8*	*16*	*32*	*64*	*128*	*256*	*512*	*Titer*
Native serum, Ro	+++	+++	+++	+++	++±	++	+±	+	±	−	256
Abs. unaltered cells	−	−	−	−	−	−	−	−	−	−	0
Abs. trypsin-treated cells	+++	+++	+++	++	++	+	±	tr	−	−	64

Serum absorbed with an equal quantity of packed cells for 1 hr. at 37°C. Trypsin treatment 15 min. at 37°C. in 4 per cent cell suspension; cells subsequently washed once with saline and packed for absorption. Titrations in saline converted to the indirect anti-globulin reaction.

TABLE 7

THE EFFECT OF TRYPSIN TREATMENT ON THE K ANTIGEN

A. *Primary reactions of anti-*K *serum and* K *positive red cells*

Technic	*Trypsin-Treated RBC*	*Unaltered RBC*
Saline	+	−
Albumin	+	+
Indirect antiglobulin	++	++±

Absorption of K *antibodies*

	2	*4*	*8*	*16*	*32*	*64*	*128*	*256*	*Titer*
Native serum, Mrs. La	++±	++±	++±	++	+±	+	tr	−	64
Abs. unaltered cells	tr	−	−	−	−	−	−	−	1
Abs. trypsin-treated cells	tr	−	−	−	−	−	−	−	1

Serum absorbed with an equal quantity of packed cells for 1 hr. at 37°C. Trypsin treatment 15 min. at 37°C. in 4 per cent cell suspension; cells subsequently washed once with saline and packed for absorption. Titrations in saline converted to the indirect anti-globulin reaction.

The Fy^a *(Duffy) Factor.* Enzyme treatment destroys the Duffy factor in a manner similar to that observed with the M-N factors. Trypsin-treated red cells absorb

The K-k (Kell-Cellano) Factors. The Kell-Cellano factors are little changed by enzyme treatment. With some anti-K sera, there is a slight degree of direct agglutination in saline observed after enzyme treatment. Absorption of K antibodies is

unaffected by trypsin treatment of the absorbing cells (TABLE 7).

The only anti-k (Cellano) serum described is a saline agglutinin that does not show much change in the k factor by enzyme treatment.

The Le^a-Le^b (Lewis) Factors. Trypsin treatment improves both Le^a and Le^b agglutination, a finding at variance with cited observations of Pickles.[12]

Normally, the stronger Lewis sera (both Le^a and Le^b), when freshly drawn, cause partial hemolysis of specific red cells. Trypsin treatment markedly accentuates this hemolysin activity (100 per cent hemolysis is not uncommon). This finding has aided us greatly in the rapid identification of these antibodies.

The P Factor. Enzyme treatment enhances agglutination with human anti-P sera, and the reactions take place over a widened thermal amplitude although not at 37°C.

False Positive Reactions. Red cells treated with enzymes in the manner described are activated at least temporarily to auto-agglutinin, similar to the "T agglutinin." Centrifugation, immediately or within 20 minutes after mixing normal serum with compatible enzyme-treated red cells, results in moderately strong agglutination (++). This subsequently disappears on incubation for one hour at 37°C.

About one per cent of individuals will agglutinate their own enzyme-treated red cells after one hour at 37°C., and the agglutination tends to become stronger with further incubation. About 0.2 per cent of sera have an appreciable titer in this manner (1:4 to 1:64). Selective absorption studies by Kellner[13] and ourselves have shown that this auto-antibody is distinct from "T agglutinin."

The agglutinogen for cold auto-agglutinins is also altered by enzyme treatment, resulting in higher titration values for cold agglutinins in general and a wider thermal amplitude. For this reason, agglutination tests with enzyme-treated cells should be incubated at 37°C. or the possibility of cold agglutination must be considered in evaluating the results.

Conclusions. The introduction of the use of proteolytic enzymes in immunohematology has been briefly reviewed. The preparation of a standard stable solution of trypsin or papain for the optimal treatment of red cells in the detection of incomplete iso-antibodies has been described. The effect of enzyme treatment on various iso-agglutinin systems has been summarized.

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TREATMENT OF *ERYTHROBLASTOSIS FETALIS* BY EXCHANGE TRANSFUSION

By G. J. BRANCATO*

The use of exchange transfusion in the treatment of *Erythroblastosis fetalis* was begun in 1945. The purpose of this paper is to review the present status of this subject, with special reference to my own experiences.

Erythroblastosis in the baby usually results when the mother is isosensitized to a blood property, most often Rh_0, present in the blood of the fetus and absent from her own blood. In an isosensitized individual, the specific antibody may occur in two forms, namely, bivalent (agglutinins) or univalent (blockers or glutinins).[1] As Wiener[2] has demonstrated, the univalent form of antibody readily and rapidly passes through the placenta, while the bivalent antibodies are effectively held back. Thus, it is the univalent antibody which is responsible for the disease, and not the agglutinin, as originally postulated by Levine *et al.*[3] If the fetus is Rh negative, the maternal antibodies will accumulate in his serum until the titers on both sides of the placenta are equal. If the fetus is Rh positive, his cells will absorb and become coated by the maternal antibodies, so that the free antibodies in his serum will, as a rule, be of lower titer than in the maternal serum. Though there are exceptions, the severity of the disease is dependent primarily on the height of the antibody titer. With very high titers, stillbirths are the rule, while, if the titer is so low that only the most sensitive tests (enzyme-treated cells or antiglobulin) will detect the antibodies, the manifestations in the baby are usually subclinical and no treatment is required.

The fact that most live-born erythroblastotic babies appear well at birth, but within a few hours or days show signs of jaundice and/or anemia and may die, has been explained by Wiener[4] by postulating the maturation after birth of a third component in the baby's serum, conglutinin and/or complement, necessary to produce clumping or lysis of the baby's cells already coated by antibody. Conglutinin is most concentrated in adult serum and deficient in the fetus and newborns,[5,6] and the role played by conglutinin is one reason why the severity of the illness is not dependent on the antibody titer alone.

The treatment of erythroblastosis has in the past been directed to the correction of the anemia. Before the Rh factor had been found, transfusions from donors taken at random were used. Later it was found that transfusion with Rh-negative blood was effective and efficient. Then it was observed that some babies died despite correction of the anemia or without becoming anemic. In such cases, *post mortem* examination generally revealed kernicterus. Wiener and Brody[7] suggested that these deaths were due to intravascular conglutination blocking the circulation to the vital organs, such as the brain and liver. The operation of exchange transfusion was then suggested,[8,9] in which the baby's blood was withdrawn and replaced by Rh-negative blood not susceptible to clumping or lysis by the maternal antibodies. By instituting such treatment immediately after birth, the occurrence of intravascular clumping is forestalled and the baby is also spared the task of removing the products of excessive hemolysis. Moreover, a single exchange transfusion takes the place of multiple small transfusions, so that the treatment is more efficient, the hospital stay is shortened and most of these babies go home with their mothers to be followed up at the office.[10] In addition, as will be shown, the mortality rate and incidence

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of neurologic *sequelae* have been reduced by this procedure.

It is frequently advantageous to combine early induction of labor with immediate exchange transfusion, when the antibody titer is high and/or there is a history of a previous erythroblastotic baby or history of a blood transfusion. Early induction of labor reduces the period of exposure of the baby's blood to antibody action, and some stillbirths are thus prevented. The optimal time for interference is considered to be two to four weeks before term. Induction by the vaginal route is preferred to Cesarean operation.

Results

To date, I have treated 29 cases of erythroblastosis by exchange transfusion, using the radial artery-saphenous vein technique. Since the prognosis has been shown to be correlated not only with the antibody titer, but also with the previous obstetrical and transfusion history, the cases have been divided into four categories as follows: (1) those without an abnormal obstetric history and/or history of blood transfusion (TABLE 1); (2) those with an abnormal obstetric history and/or history of blood transfusion (TABLE 2); (3) those with a history of erythroblastosis with recovery (TABLE 3); and (4) those with a history of fatal erythroblastosis (TABLE 4).

As was to be expected, the highest mortality occurred in the fourth category, in which two out of three patients treated died. In one of these, autopsy disclosed hemorrhage into the ventricles, whereas in the other, kernicterus developed in spite of a prompt exchange transfusion of 550 cc. of blood. The most favorable categories are 1 and 3, in which there was no mortality. Wiener and Wexler[11] reported a mortality rate of 8.5 per cent and 0 per cent, respectively, for these classes. In class 2, our mortality rate is 33.3 per cent, as compared with 15.8 per cent of Wiener and Wexler. Intracranial hemorrhage was a complication in both of our patients in this class.

Our over-all mortality rate is 13.8 per cent and that of Wiener and Wexler 17.0 per cent. In TABLE 5, our experiences with exchange transfusions are compared with those reported by other investigators. While the mortality rates reported by most investigators vary between 15 and 20 per cent, some workers report a very low mortality rate (10 per cent or less), while others report a mortality rate as high as 35 per cent. This is attributable in part to accidental statistical variations in small series of cases, in part to differences in the severity of the cases, and in part to differences in the method of treating the cases. All but a few investigators agree that there has been a substantial reduction in mortality rate in cases treated by exchange transfusion instead of by multiple small transfusions. However, Pennell[12] has claimed excellent results with simple transfusions of sedimented cells, namely, two deaths in 21 cases, or 9.5 per cent. A serious disadvantage of this method of treatment is that it cannot be used in

TABLE 1

CASES WITHOUT ABNORMAL OBSTETRIC HISTORY AND/OR HISTORY OF BLOOD TRANSFUSION

Case number	*Patient's blood type*	*Pregnancy number*	*Maternal Rh antibody titer**	*Result*
4	O Rh_1rh	2	32 congl.	Recovered
5	A Rh_2rh	2†	24 aggl. 24 congl.	Recovered
7	O Rh_1rh	4	8 congl.	Recovered
8	B Rh_1rh	2	8 congl.	Recovered
10	O Rh_1rh	2	16 aggl. 24 congl.	Recovered
15	A Rh_1rh	2	6 aggl. 8 congl.	Recovered
17	B Rh_1rh	2	4 congl.	Recovered
19	A Rh_2rh	2	48 congl.	Recovered
20	O Rh_2rh	2	48 congl.	Recovered
21	O Rh_2rh	1	48 congl.	Recovered
25	O Rh_2rh	2	12 aggl. 12 congl.	Recovered
26	O Rh_2rh	2	64 aggl. 128 congl.	Recovered
27	O Rh_0	2	8 aggl. 8 congl.	Recovered
28	O Rh_2rh	2	2 aggl. 8 congl.	Recovered
29	A Rh_1rh	3	15 congl.	Recovered

* Aggl. indicates agglutination method; congl., conglutination (albumin-plasma) method.

† Caesarean section was done five weeks before term.

TABLE 2

CASES WITH ABNORMAL OBSTETRIC HISTORY AND/OR HISTORY OF BLOOD TRANSFUSION

Case number	*Clinical history*	*Patient's blood type*	*Pregnancy number*	*Maternal Rh antibody titer**	*Result*
2	Third baby delivered at 7 months (anencephalic)	A Rh_1rh	4	12 aggl. 12 congl.	Recovered
6	Three miscarriages at 3, 5 and 6½ months	A Rh_0	4	8 congl.	Recovered
11	Second pregnancy resulted in stillbirth; fetal heart heard 2 weeks before delivery	A Rh_1rh	3†	8 congl.	Died during transfusion
12	Spontaneous abortion at 3½ months in second pregnancy	O Rh_1rh	3	24 aggl. 32 congl.	Recovered
14	Spontaneous abortion at 3 months in third pregnancy	O Rh_1rh	4	16 aggl. 16 congl.	Recovered
18	Two transfusions about 9 years ago, reaction after second	A Rh_1rh	1‡	16 congl.	Died; intracranial hemorrhage noted at autopsy

* Aggl. indicates agglutination method; congl., conglutination (albumin-plasma) method.
† Medical induction of labor at 36 weeks.
‡ Caesarean section one month before term.

TABLE 3

CASES WITH HISTORY OF ERYTHROBLASTOSIS WITH RECOVERY

Case number	*Clinical history*	*Patient's blood type*	*Pregnancy number*	*Maternal Rh antibody titer**	*Result*
1	Second baby given 5 small transfusions before recovery	A Rh_1rh	3	Not determined	Recovered
13	Third child pallid and icteric at birth; did not develop normally; died at 9 months of pneumonia	O Rh_1rh	4	16 aggl. 24 congl.	Recovered†
16	Second child icteric at birth and given several small transfusions; jaundice lasted about 6 months; baby now normal	O Rh_1rh	3	6 aggl. 24 congl.	Recovered
22	Second and third children treated with multiple small transfusions; both normal now	O Rh_1rh	4	48 congl.	Recovered
23	This baby is a sibling of the baby in case 6	A Rh_1rh	5	32 congl.	Recovered

* Aggl. indicated agglutination method; congl., conglutination (albumin-plasma) method.
† Icterus index at start of transfusion was 200 units.

TABLE 4

CASES WITH HISTORY OF ERYTHROBLASTOSIS WITH DEATH

Case number	*Clinical history*	*Patient's blood type*	*Pregnancy number*	*Maternal Rh antibody titer**	*Result*
3	Second child treated with 2 small transfusions but died on third day. Autopsy revealed kernicterus	A Rh_1rh	3	8 aggl. 12 congl.	Died on third day despite immediate exchange transfusion of 550 cc.; autopsy revealed kernicterus†
9	Second child died on third day of severe icterus; received 2 small transfusions	B Rh_1rh	3	24 aggl. 32 congl.	Died shortly after completion of transfusion; autopsy revealed hemorrhage into the ventricles‡
24	Second baby one week premature; received small transfusion but died in 15 hours	B Rh_2rh	3	12 congl.	Recovered§

* Aggl. indicates agglutination method; congl., conglutination (albumin-plasma) method.
† Female donor used in this case. Medical induction 2 weeks before term.
‡ Caesarean section one month before term.
§ Medical induction 23 days before term.

babies with a normal hemoglobin concentration, and it has already been pointed out that occasionally death will occur in the absence of anemia.

An important advantage of exchange transfusion is that it has reduced not only the mortality rate but also the incidence of neurological *sequelae* among babies who survive. Wiener *et al.* report that among more than 50 babies followed for more than two years after treatment, not one baby has exhibited neurological sequelae. Similarly, in our own series of cases, neurological *sequelae* have not occurred, while in the past as many as 10 to 30 per cent of babies recovering after simple transfusion therapy have exhibited such *sequelae.*

Technique

A variety of methods has been used for carrying out exchange transfusions. The method used by Hart[13] in 1925, namely, infusion of blood into a peripheral vein and withdrawal of blood from the sagittal sinus, later adopted by Wallerstein, has not found favor because of the danger of extradural hemorrhage. The use of the saphenous vein for the infusion and the radial artery for bleeding has been adopted by me because it is practically foolproof, the vessels used are expendable, and the dangers of thrombosis and embolism are practically non-existent. In my experience, as in that of Wiener and Wexler, there has not been a single technical failure or mortality attributable to the procedure itself. Because of the small size of the blood vessels, however, the method does require a high degree of technical skill, so that only the expert transfusionist would undertake the procedure. The easier method of catheterizing the umbilical vein suggested by Diamond[14] has, therefore, been almost universally adopted by other workers. This is a blind procedure, however, involving the *inferior vena cava* adjacent to the liver, and, in unskilled hands, operative deaths from thrombosis, air embolism, rupture of the *vena cava* with peritoneal hemorrhage, and sepsis have occurred. In addition, there have been cases in which it was not possible to aspirate blood from the catheter, so that the procedure had to be abandoned.

By any method, an exchange transfusion using 500–1,000 cc. of whole citrated blood is a long procedure, lasting one to two hours or longer, and, therefore, trying, on both the patient and the operator. Severely affected babies cannot survive such a prolonged procedure. Moreover, introduction of a large amount of citrate and calcium gluconate to counteract the tetanogenic effect of citrate, but, especially, of adult plasma containing a high concentration of conglutinin, has served in some cases to aggravate the disease. Recently, therefore, Wiener and Wexler[15] have suggested the use of packed red cells, prepared by centrifuging freshly drawn citrated blood and discarding the supernatant plasma. In this way, they have simplified and shortened the procedure, since only 100–150 cc. of blood are withdrawn and replaced by an equal volume of packed cells. In a series of 22 cases treated in this way, only two died, one of which was moribund before the transfusion, and the other died from sepsis two weeks after the transfusion. While my own experience with this modified exchange transfusion to date includes only two cases, the results were gratifying in both these cases.

The supposed advantage of female donors

TABLE 5

CRUDE MORTALITY RATE FOLLOWING EXCHANGE TRANSFUSION IN ERYTHROBLASTOSIS DUE TO RH SENSITIZATION

Investigator	*Number of cases*	*Deaths*	*Per cent mortality*
Present study	29	4	13.8
Wiener and Wexler[16]	106	18	17.0
Diamond, Allen, and Thomas[18]	347	61*	17.8
Bartel and van Loghem[19]	141	21	18.5
Tzanck, Bessis, and Buhot[20]	34	12	35.3
Raska[21]	22	4	18.2
Wallerstein[22]	41	8	19.5
Mollison and Cutbush[23]	30	7	23.3
Mustard and Fraser[24]	24	3	12.5
Molony[25]	45	13	29.0
Ginsberg and Feldman[26]	11	0	0
Shapiro[27]	6	0	0

* 19 cases of kernicterus, living or dead, are included.

claimed by Diamond[16] *et al.* has not been confirmed by other workers.[17] I have used a female donor only once myself and that baby died.

Summary

Erythroblastosis fetalis in the baby is the result of an antigen-antibody reaction, usually the Rh_0 antigen.

Exchange transfusion has the following advantages: (1) it is more efficacious and efficient; (2) it shortens the hospital stay and is less costly; and (3) it has reduced the mortality rate and the incidence of neurologic sequelae.

Early induction of labor is at times not only advantageous but imperative.

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SECTION OF MATHEMATICS AND ENGINEERING

THE EFFECT OF SPEED AND SIZE ON THE PERFORMANCE OF A MODEL SHIP PROPELLER AT OPTIMUM EFFICIENCY*

By B. V. KORVIN-KROUKOVSKY†

A naval architect is forced to depend on test results obtained on model ships and model propellers for prediction of the speed, for the specification of the power to be provided, and for the specification of speed of rotation of the propeller shafts of the full-size vessel. In this connection, he is always confronted with the question as to how accurately the full-scale performance can be predicted from the model data, or more specifically, as to what kind of data and methods can be used to make an accurate full-scale prediction. A part of this problem will be examined in this paper: that of the performance of the propeller itself, not considering the propeller-hull interaction, *i.e.*, the so-called free water performance of propellers. The problem will be further limited to a study of one particular propeller design, tested independently in three different laboratories.

Kempf[1] and Van Lammeren[2] conducted tests on model propellers of different sizes and at different speeds, and presented data on the effect of these parameters on the thrust torque and efficiency of the propellers. An examination of the changes in the over-all characteristics of the tested propellers, however, often results in a confusing picture, and sheds little light on the methods of extrapolation to full size, and on the limitation of such methods. In particular, it often leads to an arbitrary and erroneous statement of the minimum practical size of the model, as well as to the unwarranted assumption that a model of a certain larger size will well represent the characteristics of the full-size prototype. A more useful and reliable guide can be obtained from an analysis of the model propeller performance by applying the methods of existing well-developed propeller theory to assist in reducing the test data to the lift and drag characteristics of an isolated blade section. This procedure can vary in scope and complexity, depending on the nature of the experimental data, and on the completeness of the results desired. In this paper, a simple method is used to obtain the lift and drag characteristics of a single section located at 74 per cent of the maximum blade radius, and at an advance ratio V/nD at which the maximum propeller efficiency is obtained.

The Experimental Data. The propeller design chosen for this analysis is that of the motorship SAN FRANCISCO which was initially investigated by Kempf[1] in Hamburg. Recently, a model of the same size was made from Kempf's drawings, and was tested at the David Taylor Model Basin in Carderock, Md. A small model of this same propeller was also made and tested at the Experimental Towing Tank of Stevens Institute of Technology. The Stevens model was copied from the DTMB model by a pantograph attachment to a lathe. The geometric dimensions, the test speeds, and the resulting performance of the tested propellers are given in TABLE 1. A typical plot of the data is given on FIGURE 1. This customary way of presenting the data consists of plotting the measured thrust T and torque Q in the form of non-dimensional thrust and torque coefficients K_t and K_q (as defined on the chart), against the ratio of the velocity V through undisturbed water to the product of the revolution per second n and the propeller diameter D (*i.e.*, J = V/nD). Each series of tests was conducted at approximately the same RPM, with J varying with the velocity of advance V from zero to the speed at which the thrust vanishes. As can be observed from FIGURE 1, there is a

* This paper, illustrated with lantern slides, was presented at the meeting of the Section on March 16, 1951. It describes part of the research on self-propulsion tests of ship models done at the Experimental Towing Tank under contract with the Office of Naval Research.

† Experimental Towing Tank, Stevens Institute of Technology, Hoboken, N. J.

certain scatter of the data, and it is difficult to distinguish by eye the relatively small effect of the speed of rotation as shown by different coding of the four series of tests shown. In order to make the comparison as reliable as possible, a definite method of fairing by calculation was adopted, and was applied to all test data, without any regard to the visual fairing performed by the original investigators. In the case of the thrust coefficient K_t, it is observed that the curve is practically a straight line in the range from $J = .55$ to J_{max}. Furthermore, it was noted that in this range of J, the slope dK_t/dJ of the line is about uniform in all cases, and that any error in the estimation of the slope would not be important in view of the location of the finally desired points at $J = .80$, which is about in the middle of the range chosen. The equation of this part of the thrust line then becomes:

$$K_t = A - .450\ J. \qquad (1)$$

The values of A were computed for all observed test values of K_t in the range $.55 < J <$ max., and were averaged separately for each series of tests. The final faired value of K_t for J of maximum efficiency (.75 or .80) was then computed by equation 1, using the averaged values of A, and is given in the fifth column of TABLE 1.

A similar process was used for the torque coefficient; except in this case the plot of FIGURE 1 is represented by the parabolic arc

$$K_q = C - .36\ J^2.$$

The constant C was computed for each test value of K_q, averaged for each series, and the value of K_q for J of maximum efficiency was computed on the basis of this average, and is given in the sixth column of TABLE 1. By this method, the

TABLE 1

MODEL PROPELLER PERFORMANCE AT V/nD OF MAXIMUM EFFICIENCY

Designation no.	*Tested by; prop. designation; diameter;* J = V/nD *at maximum efficiency*	RPM = 60n	*Reynolds no. at* r = .74R × 10^5	$K_t = \frac{T}{\rho n^2 D^4}$	$K_q = \frac{Q}{\rho n^2 D^5}$	$T_c = \frac{T}{\frac{1}{2}\rho n^2 \pi R^2}$	$P_c = \frac{\text{Power}}{\frac{1}{2}\rho n^3 \pi R^2}$	$\eta = T_c/P_c$
1	Experimental Towing Tank,	800	.45	.105	.0249	.418	.778	.573
2	Stevens Institute of Technology;	1000	.56	.116	.0262	.462	.819	.564
3		1200	.67	.120	.0271	.478	.847	.564
4	Model No. 52;	1308	.73	.123	.0264	.489	.825	.593
5	D = .323 feet; J = .80	1400	.78	.124	.0256	.494	.800	.618
6	Kempf[1];	204	.86	.102	.0225	.407	.702	.580
7	Model No. 3542;	363	1.53	.109	.0217	.433	.678	.638
8	D = 885; J = .80	792	3.35	.124	.0246	.492	.767	.645
9	David Taylor Model Basin;	175	.80	.106	.0208	.478	.785	.609
10	Model No. 2214; D = 885; J = .75	500	2.27	.126	.0234	.570	.884	.644

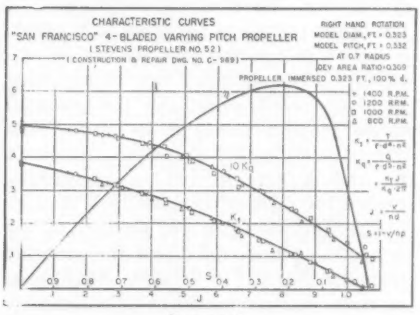

FIGURE 1.

personal characteristics of each investigator's method of fairing were eliminated, and each value of K_t or K_q at the J of maximum efficiency was found as an average of a number of test points. In the seventh and eighth columns of TABLE 1, these data are converted to another form of nondimensional coefficients, T_c and P_c, which are more convenient for use in the contemplated analysis. In the last column, the efficiency $\eta = T_c/P_c$ is given. In view of the process of the fairing used, these efficiencies do not always correspond to the ones reported by the original investigators.

The Method of Analysis. An analysis of the data will be made utilizing the modern theory of propellers, known as the circulation theory. However difficult this theory is in its original derivation, it is extremely simple in its application. FIGURE 2 shows a four-bladed propeller with a helicoidal vortex sheet trailing from the edge of each blade. Betz[3] has shown that the optimum efficiency of a propeller is obtained when the velocity distribution in the wake is such that these helicoidal vortex sheets trail aft without change of form, *i.e.*, as if they were rigid material sheets. Prandtl[3] made the first approximate calculation of the velocity distribution needed to satisfy this condition, and Goldstein[4] developed the final rigorous solution.

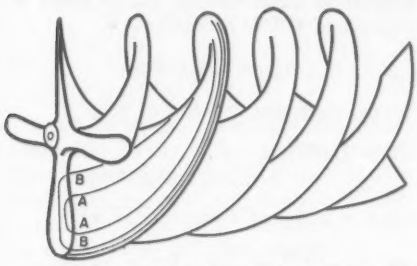

FIGURE 2. Diagram showing helicoidal vortex sheets in the wake of a propeller.

A specified velocity distribution in the wake implies a specified distribution of the thrust along the blade, and one speaks, therefore, of Goldstein's (or Betz's) thrust distribution. Strictly speaking, the circulation theory applies only to propellers designed to provide this thrust distribution, and only to their performance at the speed of maximum efficiency. In practice, it is found, however, that the maximum efficiency is affected very little by a certain amount of deviation of the thrust distribution from the optimum, which thus implies a certain range of deviation of the ratio V/nD from the optimum. Little error will be involved, therefore, in the inverted application of the theory to the analysis of a given propeller, in assuming that the propeller operating at the maximum efficiency conforms to the ideal thrust distribution. With this assumption, the analysis can proceed following the elementary Rankine-Froude momentum theory, except that the uniform velocity of the wake will be replaced by the mean velocity u_{zm} as computed by Goldstein.

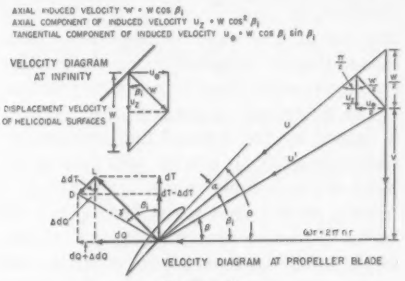
AXIAL INDUCED VELOCITY w = w cos β1
AXIAL COMPONENT OF INDUCED VELOCITY uz = w cos² β1
TANGENTIAL COMPONENT OF INDUCED VELOCITY uθ = w cos β1 sin β1
VELOCITY DIAGRAM AT INFINITY
DISPLACEMENT VELOCITY OF HELICOIDAL SURFACES
VELOCITY DIAGRAM AT PROPELLER BLADE

FIGURE 3.

First, reference is made to FIGURE 3, giving the velocity and force vector diagrams at the propeller blade and also the velocity vector diagram at the surface of the helicoidal sheet at an infinite distance aft of the propeller, where the flow in the wake becomes steady. The horizontal vector ωr is the tangential velocity of a blade element due to the velocity of rotation; the vector V is the velocity of advance and U′ is the resultant velocity of a blade element with respect to the undisturbed fluid. The reaction to the lift force L is an

induced velocity vector w/2 produced at the blade, so that the actual velocity of the water with respect to the blade is shown by the vector U. The angles of the vectors U′ and U with the plane of the propeller are designated by β and β_i. At the propeller blade, the vector U is tangent to the helicoidal vortex sheet, which by definition floats with the fluid, *i.e.*, moves normal to itself with the induced velocity w/2. At an infinite distance aft of the propeller, the induced velocity increases to w, and, since its axial and tangential components u_z and u_θ increase in the same proportion, the helicoid rotates as it moves aft without change of shape. In making this statement, it is assumed that the diameter of the helicoid remains equal to the propeller diameter, or in other words, that the contraction of the wake is neglected. This assumption is permissible for moderately loaded propellers used in common practice.

The movement of the helicoidal surfaces is caused by the induced fluid velocity w, and, conversely, it can be said that movement of the helicoid causes the velocity w. Since a perfect fluid is considered, and the forces of friction do not exist, the velocity of the helicoidal sheet along its surface will cause no motion in the fluid. This being the case, it is convenient to replace the vortex sheet velocity w in a direction normal to itself by the velocity of the displacement W along the direction z parallel to the propeller axis. In either case, the fluid will have the velocity w with components u_z and u_θ, but these are now expressed in terms of W as

$$\begin{aligned} u_z &= W \cos^2 \beta_i \\ u_\theta &= W \cos \beta_i \sin \beta_i \end{aligned} \tag{2}$$

Furthermore, the velocity in the wake is not uniform, and it was shown by Prandtl[3] and Goldstein[4] that the fluid velocity is smaller in the space between the helicoidal sheets than at the surface. The mean axial component u_{zm} of the fluid velocity is, therefore,

$$u_{zm} = Ku_z,$$

where the constant K was originally computed by Goldstein,[4] and then the computations were extended by Lock[5] and Kramer;[6] they are used here as reproduced by Theodorsen.[7]

The Rankine-Froude axial momentum relations can now be written. Consider the area contained between radii r and r + dr:

$$2\pi r\, dr \tag{3}$$

Fluid mass flowing through the above area:

$$2\pi\rho(V + \tfrac{1}{2}u_{zm})r\, dr \tag{4}$$

Momentum added = thrust increment:

$$dT = 2\pi\rho(V + \tfrac{1}{2}u_{zm})u_{zm} r\, dr \tag{5}$$

Expressing mean velocity u_{zm} in terms of displacement W of helicoids:

$$u_{zm} = KW \cos^2 \beta_i \tag{6}$$

Thrust increment in terms of W:

$$dT = 2\pi\rho[VWK \cos^2 \beta_i + \tfrac{1}{2}(WK \cos^2 \beta_i)^2]r\, dr \tag{7}$$

Let r = x R, and divide by $\frac{1}{2}\rho V^2 \pi R^2$:

$$dT_{ei} = 4\left[\left(\frac{W}{V} K \cos^2 \beta_i\right) + \tfrac{1}{2}\left(\frac{W}{V} K \cos^2 \beta_i\right)^2\right]x\, dx \tag{8}$$

The thrust can be obtained now by integration of dT (x) from x = 0 to x = 1, *i.e.*, over the length of the blade. A shortcut is available, however, in that, in practice, the distribution of the thrust along the blade is found to be sufficiently uniform for different propellers so that the integral can be evaluated on the assumption that

$$T_{ei} = \int_0^1 dT(x) = \frac{dtc/dx_{atx=a}}{\text{constant}} \tag{9}$$

On this basis, the performance of a propeller is often judged by the performance of a single section at x = .7 or x = .75. In the

present case, the analysis is based on x = .74, since a section was shown on Kempf's drawing at r = 100 mm, or r = .74R. The interpolation of the data given for this type of analysis by Lock,[8] Driggs,[9] Hartman,[10] and Lerbs[11] leads in the present case, to an estimate of the constant as being 1.63, so that:

$$T_{ci} \approx \frac{4}{1.63}\left[\left(\frac{W}{V} K \cos^2 \beta_i\right) + \tfrac{1}{2}\left(\frac{W}{V} K \cos^2 \beta\right)^2\right] \quad (10)$$

The ideal efficiency of a propeller is defined as

$$\eta_i = \frac{1}{1 + W/2V} = \frac{\tan \beta}{\tan \beta_i} \quad (11)$$

and

$$P_{ci} = T_{ci}/\eta_i \quad (12)$$

In the Rankine-Froude linear momentum theory, the velocity W in equation 11 above was understood to be the true axial velocity of the uniform wake, and the only energy loss was considered to be in connection with the increment of the axial velocity.

In the circulation theory, W is understood to be the fictitious "displacement velocity" of helicoids, greater than the mean axial velocity u_{sm}, and, as such, it includes the effects of energy losses of velocity in tangential and radial components, as well as in the axial component.

The subscript i in the above designates the ideal efficiency, *i.e.*, the efficiency obtained by neglecting the profile drag of blade sections. The values of T_{ci}, P_{ci}, and η_i are uniquely determined by the displacement velocity ratio W/V. On FIGURES 4 and 5, the upper curves designated $\eta = 0$ represent plots of the ideal efficiency η_i *vs.* thrust disk loading coefficient T_{ci} or power disk loading coefficient P_{ci}. The displacement velocity ratio W/V is indicated by the dotted parametric lines. This curve of ideal efficiency agrees closely with similar (and greater in slope) curves published by Kramer[6] and Lerbs.[12]

As shown on FIGURE 3, the effect of the profile drag D is to reduce the thrust and to increase the torque of a blade element.

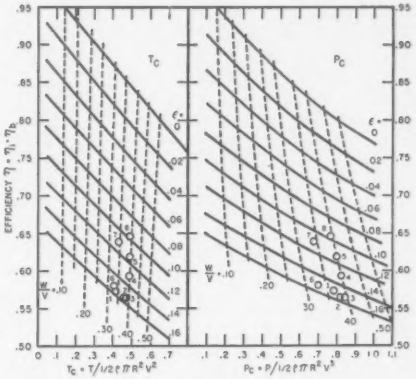

FIGURE 4. Maximum efficiency *vs.* T_C, P_C, and ϵ J = .80.

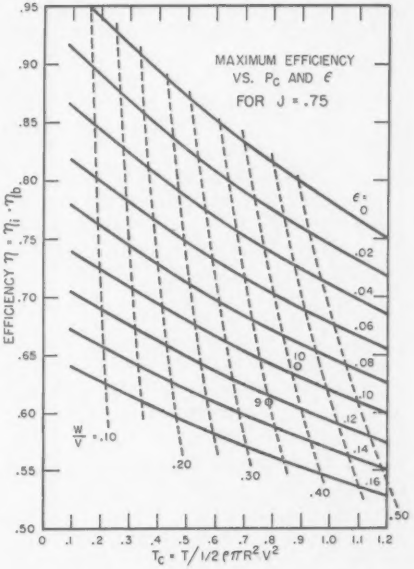

FIGURE 5.

Bienen and Karman[13] have shown that the integrated effect for the entire blade length can be put into the form of a correction to

the ideal thrust and torque coefficients, so that the actual coefficients become:

$$T_c = T_{ci}(1 - Z\epsilon \tan \beta_0) \quad (13)$$

$$P_c = P_{ci}\left(1 + \frac{2}{3}\frac{\epsilon}{\tan \beta_0}\right) \quad (14)$$

where $\epsilon = C_D/C_L$

$\tan \beta_0 = \tan \beta_i$ at r = R (*i.e.*, at the tip of the blade)

The blade efficiency is

$$\eta_b = \frac{1 - 2\epsilon \tan \beta_0}{1 + \frac{2}{3}\frac{\epsilon}{\tan \beta_0}} \quad (15)$$

and the total efficiency:

$$\eta = \eta_i \eta_b \quad (16)$$

Parametric curves of the total efficiency η are plotted on FIGURES 4 and 5 for a number of values of the drag/lift ratio ϵ.

Finally, the lift coefficient of a blade element is given as[11, 15]

$$C_L = \frac{\kappa}{\sigma} 4 \sin \beta_i \tan (\beta_i - \beta) \quad (17)$$

where

κ = a coefficient related to K, and is tabulated by Lock in reference 5.

σ = the solidity defined as $Nb/2\pi r$, where N = the number of blades, and b = the chord of a blade at radius r.

Analysis of the Blade Section Drag. The analysis of the propeller performance now becomes very simple. The observed efficiency taken from TABLE 1 is plotted *vs.* T_c and P_c on FIGURES 4 and 5. The W/V ratio at which the propeller works, and the $\epsilon = C_D/C_L$ for the blade element are read directly from the relation of the plotted point to the parametric lines W/V = const. and ϵ = const. With W/V known, the $\tan \beta_i$ is determined as

$$\tan \beta_i = (1 + \frac{W}{2V})\tan \beta$$

$\tan \beta$ is known, since $\tan \beta = V/\omega r$.

With β and β_i known, the lift coefficient is computed by equation 17 and the drag coefficient C_D is computed as ϵC_L.

The drag coefficients C_D found in this way are plotted *vs.* Reynolds number on FIGURE 6 for the three tested propellers. All ten test conditions are plotted and compared with three standard friction resistance curves. Although there is a considerable scatter of data, there is a clearly defined tendency to follow an average line, essentially parallel to the lines expressing the frictional drag for the turbulent condition of the boundary layer. The mean profile drag of the section at .74R of the propeller design under consideration is found to be equal to two times the friction drag of a flat plate having the same pro-

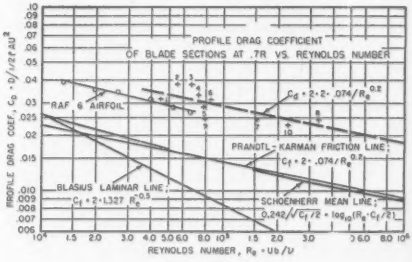
PROFILE DRAG COEFFICIENT
OF BLADE SECTIONS AT .7R VS. REYNOLDS NUMBER
RAF 6 AIRFOIL
PRANDTL - KARMAN FRICTION LINE;
BLASIUS LAMINAR LINE;
SCHOENHERR MEAN LINE;
PROFILE DRAG COEF.
REYNOLDS NUMBER, Re = Ub/ν

FIGURE 6.

jected area. Since this factor of 2 is unexpectedly high, the wind-tunnel test data of an old design airfoil RAF6, tested in a very turbulent type wind tunnel at low Reynolds number, are plotted on FIGURE 6 for comparison. The two sets of data agree as to the order of magnitude, and, in fact, the direction of the plotted curve indicates a turbulent condition of the boundary layer. This condition for an airfoil is probably brought about by the turbulent condition of the wind tunnel, and by the type of airfoil section possessing a harsh curvature at the leading edge. The propeller blade section is described by the ordinates given in TABLE 2, and appears to be of the Joukowsky type with circular arc median line. When tested at a towing tank, each blade encountered undisturbed water as free of

turbulence as can be expected. It was surprising, as well as gratifying, therefore, to find the turbulent boundary layer type of resistance curve, gratifying because it permits a convenient extrapolation to any value of the Reynolds number, including the full size.

In looking for reasons to explain the occurrence of a turbulent boundary layer at such a low Reynolds number, a hypothesis has been advanced that the turbulence is probably stimulated by the vibration of the rapidly rotating propeller model.

TABLE 2

ORDINATES OF THE PROPELLER SECTION AT .74R

(SCALED FROM KEMPF'S MODEL DRAWING, STATION AT r = 100mm.)

Distance X, in per cent of chord	*Ordinates y, in per cent of chord*			
	Lower	*Upper*	*Median line*	*Half thickness*
0	0	0	0	0
1.25	− .64	1.38	.37	1.01
2.5	1.06	2.32	.63	1.67
5	−1.48	3.44	.98	2.46
7.5	−1.72	4.18	1.23	2.95
10	−1.90	4.87	1.49	3.39
15	−1.90	5.87	1.99	3.89
20	−1.80	6.45	2.33	4.13
30	−1.00	7.24	3.12	4.12
40	− .13	7.40	3.64	3.70
50	.27	7.25	3.76	3.50
60	.53	6.60	3.57	3.02
70	.48	5.55	3.01	2.54
80	.16	4.02	2.09	1.93
90	− .05	2.22	1.09	1.14
100	0	0	0	0

Radius trailing edge .9
Computed angle of zero lift—about 4.3 degrees

A test was made in the condition corresponding to Test 4, in which the model propeller was made to operate in the turbulent wake of a series of round rods towed in front of the propeller. The relation of T_c and P_c in this test remained the same as in Test 4, indicating that added turbulence had no effect on the drag. This fact can be taken as supporting evidence that the boundary layer was already turbulent.

Lift of the Blade Sections. The lift coefficient was computed by equation 17, and was corrected to represent the section as it would be tested in the wind tunnel by applying "curvature correction" as developed by Hill.[14] No wind-tunnel data for the section in question were available, but the lift characteristics to be expected can be estimated by the computational methods known from the theory of airfoils. The lift coefficient as tested was found to be very much lower than indicated by this estimate. Again a comparison was made with the low Reynolds number tests of an RAF airfoil, and it became apparent that the low value of the lift coefficient is due to the decrease in the absolute value of the angle of zero lift at small Reynolds numbers. The angles of zero lift as computed from the test data are shown on FIGURE 7, where the asymptotic value for the larger Reynolds number is also shown. The mode

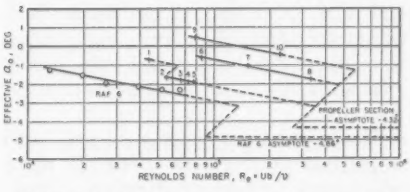

FIGURE 7. Angle of zero lift a_0 plotted *vs.* Reynolds number.

of the approach to this asymptotic value is, however, not known. It appears that smaller, more rapidly rotating propellers have a smaller deficiency in the angle of zero lift than larger propellers compared at the same Reynolds numbers. This checks with the data of Van Lammeren,[2] where, at a given Reynolds number, smaller propellers showed higher efficiency than the larger ones.

The propeller used for Tests 9 and 10 shows an abnormally large deficiency in the angle of zero lift. It also shows the best efficiency V/ND at a value of J = .75, instead of at J = .80, which was common for the other tested propellers, indicating a reduced strength of the circulation. The finish of the blades appeared to be unusually rough, but no data are available as to the effect of the roughness on the circulation,

and on the resulting change in the angle of zero lift.

Summary. The analysis of the test data for model propellers on the basis of the circulation theory explained their performance in terms of the lift and drag characteristics of the blade sections. The drag characteristics followed the turbulent boundary layer law of skin friction, so that extrapolation of drag coefficients from model to full scale should present no difficulty. This conclusion refers only to the specific blade section analyzed in this paper. There is evidence in Van Lammeren[2] tests (not yet analyzed in detail) that a mixed laminar-turbulent flow exists in small propeller models with the low drag type of blade sections of a more modern design. The attempts by the Experimental Towing Tank and Van Lammeren to use an artificial turbulence stimulation were inconclusive, since the models being tested were already (unknowingly) in a developed turbulent condition.

The most serious discrepancy was found in the values of the angle of zero lift, which will require further investigation. Tentatively, it appears that smaller model propellers should be made with a degree or two larger pitch angle than the full-size prototypes in order to give the same performance. Again, this appears to apply to the particular propeller analyzed. In an example given by Lerbs,[11] a good agreement of lift coefficient was obtained on a very thin ogival section at a somewhat greater Reynolds number.

The range of Reynolds numbers between the "small" and "large" models is rather narrow, and even the "large" models cannot be assumed to represent the full-size performance directly.

If this type of analysis had been applied to the propeller when it was designed, it would have clearly indicated the possibility of greatly improved performance by reducing the profile drag losses, preferably by using a more favorable blade section, or by reducing the number of blades from four to three with a corresponding increase of pitch (*i.e.*, the angle of attack and of the lift coefficient).

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SECTION OF PSYCHOLOGY

THE ADJUSTMENT OF THE BLIND*

By HECTOR CHEVIGNY†

The trend of scientific thought today is to seek more for similarities among men than differences. We approach the study of personality today in the general belief that when all factors are understood, no element will be altogether foreign to any of us. Some feel that the swing from emphasis on the constitutional-heredity factor to the environmental in the estimate of men has been too wide and free, but, it seems to me, this change has worked to establish a sense of kinship among us all. So long as we clung, for example, to Lombroso's views in criminology, we were hindered from understanding that the criminal was an individual worked upon by forces to which any of us, given the wrong circumstances, might have succumbed.

I make these somewhat commonplace observations to highlight the fact that, when it comes to the field of thought concerning physical handicap, we have remained so firmly allied to the constitutional-heredity concept that it is almost as if we had never left Lombroso behind. It is common to speak of "the polio personality," "the personality of the deaf," and the like. Although it is widely recognized that blindness, deafness, hunchback, and other easily-discernible physical handicaps arouse strong emotions in others, we have done little work so far in estimating how much of the alleged personality differences concomitant with these handicaps are due to the handicaps as direct, primary influences and how much may be traceable to reciprocal social effect.

Probably no category of handicap seems so determinative in creating a personality difference of itself as lack of sight. The phrase "adjustment to blindness" connotes a variety of human experience with which we all seem to feel so little relationship that, instead of inspiring ready understanding, it instills a feeling of wonder and bafflement. Mankind has always manifested surprise that the blind manage to adjust at all to their condition and, when they demonstrated the ability of the human creature to function (and sometimes to function well) without sight, recourse was had to the fabulous and even the supernatural for explanation. A body of singularly fact-resistant fable and fallacy has existed for thousands of generations concerning the mode by which the blind adapt physically to achieve movement, and this body of fable extends even to the mental and emotional life of the blind.

The body of fable held, in essence, that physical adaptation to blindness was achieved by the development of superacuity in the remaining sensorium by a process of "compensation." Hearing and, particularly, touch were said to grow not merely in perceptive keenness but in sensitivity. Stories about the abilities of some of the blind to hear sounds others cannot hear, to detect colors by feel, to read newspaper headlines with supersensitive fingertips, even to distinguish the numbers on money-bills, are too familiar to need amplification here.

The abilities of some well-oriented blind individuals to make what may seem to be quite astonishingly correct estimates of their distance from obstacles, the approximate size of cards placed before their faces, and so on, seemed to document the fable. There is hardly an Eighteenth Century philosopher who has not seemed, at one time or another, to speculate about the origin of this ability, and it preoccupied the psychologists greatly around the time of the birth of their science. Many blind individuals, indeed, most of them, as they do to this day, reported that on nearing such obstacles as walls and the like they felt definite sensations in the facial tissues. The term "facial vision" came to denote this phenomenon, as it still does. It was assumed that the skin was involved in the

* This paper was presented at the meeting of the Section on March 19, 1951.

† Thirty-four Gramercy Park, New York, N. Y.

perception process, no doubt by dint of the development of supersensitivity within it.

So deeply rooted in the imagination was the "compensation" view that, at the beginning of their researches about all this, the psychologists took superacuity for granted, treating it not as something to be investigated but as a premise upon which a structure of explanation of its existence was to be built. The attempt was almost deliberate in building, for all sightless people, a "psychology of the blind." Some of the conclusions were pretty fantastic, more so than the phenomenon they investigated. Talk of the sixth and even seventh sense of the blind was common, and even such great men as Wundt and Macdougall were guilty of using the terms. As late as 1928, the French researcher Romaines came out with the theory that, upon loss of sight, the skin may develop, from vestigial nerve corpuscles in the skin, little "eyes" which actually can see. Levy propounded the theory that throughout the sensorium there is a certain quantum of energy which, when partially dammed up by loss of one sense, backs up and flows the more strongly through the remaining avenues of perception.

It is remarkable how even the development of controlled laboratory methods failed for a long time to establish the truth. Finally, such men as Seashore demolished for good the fable of superacuity, by a statistical evaluation of experiments on both blind and sighted individuals, particularly children. Testing all phases of alleged superacuity, not merely the tactual element but also sensitivity to heat, cold, and the like, Seashore ascertained that the mistake had always lain in confounding superacuity with mere developed perception born of a mechanism familiar to us all and peculiar to the blind only in its application. Thus, it was not until our own times that the first scientific step was ever taken to relate the physical adjustment process of the sightless to a general rather than a special psychology. But the fables die hard. It seems at times that virtually every young psychologist who gets hold of a blind student is impelled to set to work to establish superacuity, and, it has also been my observation, that, when he cannot do so, he is likely to be disappointed. Throughout even the very authoritative psychiatric literature, one constantly encounters such phrases as "the well known disposition of the blind to develop unusual keenness of sensory perception."

The fable of facial vision did not topple until 1943. It was then established with experimental certainty that hearing alone (the experientially-developed ability to judge accurately of distance by sound-wave reflection) was responsible for the phenomenon, and that feelings in the skin were reactive, secondary expressions of the awareness of danger from obstacles. At Cornell, Dahlenback, Supa, Cotzin, and associates took long-blinded individuals, matched them with blindfolded sighted people and, over a period of days, made a series of tests in an experimental hall. These tests were simple and consisted of approaching and receding from stationary and movable obstacles while individually-expressed reactions and estimates of distance were checked. This part of the experiment was not new, having often been performed previously, and with results which were inconclusive in so far as ruling out the skin as an awareness-area was concerned. The Cornell experiments added new wrinkle—telephony. After the first routine of tests, the subjects were removed from the experimental hall, given headphones hooked to equipment carried by the experimenter who, in the hall, repeated the paces gone through by each subject. The estimates of distance made by microphone proved substantially so similar that the skin could be statistically ruled out as a factor, for only hearing could be involved in the telephonic experiment.

All this was of more than academic interest. There is a well-grounded belief that the adaptive process in the blind can be hastened by training. But, obviously, we must understand the dynamics of the

adaptive process. The Cornell experiments received what amounts to clinical confirmation in the armed forces' work in retraining blinded veterans, which was the first such attempt on an organized scale of which we have any record in America. The blinded veterans' training began at Valley Forge Hospital and continued in a remarkable convalescent center set up at Avon, Connecticut. My time is too limited to go into details of practice. Many courses of various kinds were given the veterans, but prominent among them was what has come to be called Orientation (training in awareness of distance by the estimation of sound-bounce from obstacles) and general safety in foot travel. Older blind people estimate as a rule that the length of time needed for the ordinary blinded adult of good physical capacity to "learn to be blind," as they put it, is an average of two years. The work at Avon showed that if newly-blinded people are given good training quickly, before bad habits have set in and inertia has taken the place of ambition, this traditional two years can be cut to as little as six weeks.

The results at Avon have been declared inapplicable to the majority of the blind. Certainly, most of the veterans were an ideal age for a severe adaptative demand; of good average intelligence and emotional balance; and, because they were almost all blinded accidentally, with little collateral pathology to obscure their recovery picture. Nevertheless, the need for experimentation is too urgent for mere academic debate on the subject of applicability of training. The key to the economic and often the emotional situation of a blind individual is his independence and ability to deny the allegation of complete helplessness. Avon was dissolved after the last batch of war-blinded went through it. Its work survives, having been influential in the formation of a few training centers under the wings of social agencies for the civilian blind, but there is fear for its continuation. Plainly, it is work that takes great skill, understanding, and emotional stability, and Avon did not exist long enough to graduate a large corps of experts. The civilian centers are unfortunately looked upon with a certain languor by their parent agencies, and it is probable that what vitality the movement is showing is largely the result of the efforts of the Federal Office of Vocational Rehabilitation. Properly, all of this is a branch of physical medicine, but work in which the psychologist can play a most important role.

I said, at the beginning of this paper, that the fables and fallacies extend to the mental and emotional structure of the blind. These the experimental psychologist thought inaccessible to his mode of inquiry and left them to the clinician. The clinician has been extremely slow in doing much about it. The first to do so received a blow which discouraged many others from trying it. In 1932, Dr. Thomas Cutsforth, of Los Angeles, a practicing clinical psychologist and himself a product of our school system for the blind, published a book titled *The Blind in School and Society*, in which he showed the physiological improbability of most of the emotions credited to blindness and charged that most of the mischief wrought in the personality of the blind did not come primarily from their handicap but secondarily, through a mobilization of effect in the social environment. He also made some very shrewd and cogent observations on the methods of teaching in the schools for the blind, pointing out that they still trained the children on the old "compensation" theory. His work caused a tremendous uproar from educators and heads of social agencies, and its suppression was demanded. His book is now, however, looked upon as the pioneer effort in a new understanding which is actually taking the form of a social psychology.

I need hardly prove how deeply imbedded is the belief that the blind live in "a dark world." Getting accustomed to an absence of light and to the positive awareness of darkness is popularly said to be one of the most difficult elements in adjusting to blindness. I know of few ideas concerning handicap more firmly fixed in our language.

The phrases "the dark world of the blind," or "seeing light," as a synonym for returning sight, trip from our tongues as unconsciously in speaking of the blind as we use the words "and" and "but." Among one hundred titles of books on the blind, I counted eighty-nine which contained the words "dark" or "light." Braille publications and other magazines for the blind are persistently titled by their sponsors "The Gleam," "The Sunbeam," "The Glow." The world's largest social agency for the blind is called The Lighthouse. So far as I know, Dr. Cutsforth was the first to show the physiological absurdity of the concept and the mischief its survival actually wreaks in the lives of the blind. Cutsforth showed that perpetual awareness of darkness is impossible, what we think to be darkness quickly being replaced by the photisms of the imagination.

As with phrases which Jews and Negroes find mischief-wreaking to themselves, and non-Jews and non-Negroes may not understand to be harmful until the reason is explained, so blind individuals with a strong awareness of the operation of majority psychologies are very resentful of the language containing the darkness concept. It is entirely a projection by the majority of its ideas about blindness, and these ideas are invested with feelings of gloom, melancholy, and horror. The darkness concept is much more than a verbal accident, a harmless euphemism.

I wish it were possible to review here, even cursorily, the history of the blind over the past two thousand years. It furnishes fascinating documentation to some of the views of the founders of depth psychology. That there was real emotional fear of the blind themselves, and not merely of blindness, is the most discernible element of the history. The distinguishing feature of the life of the vast bulk of the blind throughout the ages (and the few who lived otherwise only highlight the lot of the others) is the segregative existence imposed on them. The almost stubborn disposition not to allow the blind to labor is very curious to contemplate. Beggary was for the most part their lot and, in some countries, it was reserved to them by edict. Various cultures gave various rationales for their behavior. The Mohammedan world, as a whole, held that the lot of the blind was hard enough without having the need to work also thrust upon it. Parts of the Hindu world held that blindness was clearly a sign of sin and so to allow the blind the privilege of labor was impious. Only in Japanese culture do we find a conspicuous exception, and to explain it is a puzzle. In Japan, from the middle of the Ninth Century, the blind of all economic degrees and birth have had social rank and standing, and definite professions to follow.

Even during those crude ages when physical handicap was considered funny and people with hunchbacks were the court clowns, one finds surprisingly little evidence that the blind were thought amusing. They were certainly entertainers, historically speaking, but they dealt in beauty, in poetry, and in music. The treatment accorded the blind has always been in startling contrast to the treatment accorded the deaf, and the popular notions of the stereotypic personalities of the blind and the deaf differ accordingly.

As anyone who has spent even a few years in the entertainment business can testify, jokes about the blind need no rules against them in radio, television, vaudeville, or any other branch. They are simply never made. But the rules against jokes about deafness are many. Now, this is of more than passing interest. It becomes increasingly interesting if we consider the separate roles of sight and hearing in sex and the kind of emotion popularly associated with blindness.

If one includes in man's sexual apparatus the elements of the sensorium which cause it to direct its aims and locate its objects, sight may be easily seen to fill an important role. Few would contest the remark, I think, that man does a large part of his sexual choosing by visual values. But, is sight more than a channel of stimulation,

however important? Is its importance merely a difference of degree, or is it also a difference in kind of importance? As Krafft-Ebing and other observers long ago noted, man may develop sexual perversions based on sight (witness the Peeping Tom, the voyeur). Freud denominated the visual field an erogenous zone, capable of becoming, under developmental stress, an area in which sexual play may take place divorced from proper aim or object.

Individuals do seem to develop the tendency to find complete sexual satisfaction, that is, complete in so far as further extension of activity is concerned, in looking at displays having sexual significance. Hearing, although plainly a channel of sexual stimulation, does not seem to play a role of the same kind in man. Has any of us ever heard of a Hearing Tom? It does not seem to be established that sounds themselves, in the way certain sights do, ever become equivalents for sexual satisfaction. Much sexual interchange is expressed verbally, to be sure, but it is a real question whether the stimulation thus obtained is not first converted to visual imagining. We may be sure, however, that in the human creature the sound of the voice (which concerns us here more than its verbal content) is of less importance in the selection and appreciation of the sexual object than is its visual aspect.

Mythology and dreams are replete with symbolism equating sight and potency. Columbia University's Dr. Henry Hart has a fascinating paper titled "The Eye in Symbol and Symptom" which I suggest to the more curious about this subject.

Crude and brief as has been my establishment of a chain of reasoning here, perhaps I can step with safety to our conclusion, which is that loss of sight induces a very direct mobilization of castration fear. All physical deprivation mobilizes this fear, but with sight playing so singular a role in the syndrome of sex stimulation, the fear blindness engenders is of a vital nature. To this may be traced much of the emotional burden of the ideas entertained by man concerning blindness and the blind and his historic tendency to isolate them. I disagree with some that this is the entire explanation of the troubles of the blind with their environment, but accepting it as a very large part of the explanation removes most of what is indeed a deep puzzle.

Projections of castration fear directly contribute to our general misunderstanding of what would be, without this element, the most difficult part of adjustment to blindness to understand. This is the so-called "emotional adjustment." That anyone who loses sight has an emotional adjustment to achieve is not being questioned here. What is in question is the degree to which emotional adjustment to loss or lack of sight is fable and how much is truth. It is everywhere held that the emotional adjustment which every blind person must make may influence, if indeed it does not entirely inhibit, physical adaptation. We hear it said, "He never climbed out of it," and "It was too much for him, he gave up."

Our language, our literature, adopts the view so wholeheartedly that certain emotions must be associated with blindness that nobody seems to be combative enough to define them. Summarizing the general belief, it comes down to this: that loss of sight engenders a feeling of loss akin to grief, that this melancholia is permanent and pervasive in the personality, that humor is an offense to it, and that denial of its existence is evidence of a very great and courageous soul indeed. If we turn to textbook descriptions of involutional melancholia, we note a marked similarity to the popular notions about feelings under blindness. I probably need not explain to this company how large a psychogenic element is in the idea of the end of sexual potency in the involutional syndrome.

On strictly physiological grounds, it is doubtful if an individual, even with a supporting persistent syndrome of melancholy, could endure for long the emotions credited to blindness, but we have other data casting doubt on the genuineness of the allegations.

Suicide, for one thing, is notably low among blind individuals. So, whatever its roots may be, we may be certain that in the thought of blindness something is mobilized in society which, unfortunately for the blind, creates a psychology which becomes, upon examination, allied in its mechanical workings to the allegedly peculiar psychologies of all minority groups. By rebellion or conformity to the body of fixed notions and fables, the individual reactively expresses his kinship with the group.

I have said I do not share the view that the castration-fear element explains everything. Much data in the history is clearly expressive of a belief that loss of sight deprives the intellect of an essential element. My time is now so short, I will do no more than baldly state that man has a habit o confounding his faculty of understanding with his imagination. His imagination being, by the very word used to connote it, visual, he confounds it with his sight. He thus achieves an equation of sight with understanding. Belief that the blind child was incapable of education except at work with his hands (and at absurdly simple tasks, at that) long inhibited the spread of the schools for the blind. To this day, as some charge, the schools for the blind educate on the basis that they must compensate for a missing faculty and not to promote coordination of the faculties as they exist in the child.

By comparison with most mammals, man's hearing and sense of smell are poorly developed. Generally speaking, man's superiority in sensorial equipment lies in kinaesthesia and sight. Very sight-centered in all his activities, and preoccupied with maintaining his erect posture, loss of sight imposes a heavy task on the adaptative capacity. It would be less of a task, however, were it not for the social situation invariably surrounding the attempt to adapt. Directly inhibitory in its usual working, the environment more often than not succeeds in creating out of the blind individual precisely what its stereotypic picture about the blind holds—a completely helpless individual, emotionally and mentally wrecked. It would be describing the situation accurately to say that most blind individuals fail to surmount the difficulties imposed by their environment, not the difficulties inherent in their condition.

Much of the fabulous structure remains to be demolished. The experimental psychologists, as if disappointed that they could not establish what their own sharing of the world's fixed notions told them must surely exist in the blind, have largely turned away from the problem. We need experimental work, for we yet do not know for a certainty the relation of awareness through touch to visualization. Also, we do not have as yet the key to the mental organization of the congenitally, totally blind child. But most of all we need the attentions of the clinician, and we need young people interested in the social sciences who will animate with a new spirit of inquiry the old and famous social-service structure of care for the blind.

DIVISION OF ***MYCOLOGY***

HOMOTHALLISM AND HETEROTHALLISM IN *GLOMERELLA**

By LINDSAY S. OLIVE†

The terms homothallism and heterothallism were originated in 1904 by Blakeslee[1] in connection with his discoveries on the Mucorales. Blakeslee found that some of these molds produced zygospores readily in single spore cultures and he called these homothallic. Other species were found to produce zygospores only when two different strains, which were called (+) and (−), were mated. These he called heterothallic.

* This paper, illustrated with lantern slides, was the first of two presented at the meeting of the Division on March 23, 1951.

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Edgerton,[2, 3] in 1912 and 1914, reported a crossing of different strains in the Ascomycete *Glomerella*, one strain producing abundant fertile perithecia when grown alone and the other producing mostly infertile perithecia. Dodge,[4] in 1920, reported the first clear-cut case of heterothallism in the Ascomycetes. He showed that (+) and (−) strains were present in *Ascobolus magnificus* and that the fruiting bodies of this fungus were produced only when the two different strains were crossed. Kniep,[5] in 1915, and Bensaude,[6] in 1918, demonstrated the occurrence of heterothallism in the Basidiomycetes, a group which, like the Phycomycetes and Ascomycetes, also contains many homothallic species.

Since these discoveries, it has been rather generally believed that the fungi which reproduce sexually are either homothallic or heterothallic. However, there have been a number of interesting reports of fungi which do not fit well into either of these categories. *Glomerella* is one of these.

Edgerton,[2, 3] in his earlier investigations of *Glomerella*, studied two cultural types which he designated as "plus" and "minus" strains. The "plus" culture produced fertile perithecia when grown alone, while the "minus" culture produced perithecia with very few ascospores. It was noted that dense lines of perithecia were formed between the two cultures when they were grown together. A considerable number of asci isolated from perithecia in the lines were found to contain "plus" and "minus" ascospores. Edgerton believed that the condition existing in *Glomerella* was somewhat intermediate between the conditions (of homothallism and heterothallism) reported by Blakeslee for the mucors.

The more recent papers of Edgerton and his associates[7–14] at Louisiana State University, and those of Andes[15] and Markert[16] have brought to light a number of very interesting phenomena which strongly indicate that *Glomerella* will become one of the most useful of the lower organisms for genetic study. The most recently published paper on *Glomerella* which has come to the writer's attention is that of Struble and Keitt,[17] whose investigations on variability and inheritance in the fungus confirm many previous observations and add important new data. In the present discussion, no attempt is made to go into all the ramifications of the *Glomerella* problem. However, certain features of the work which are considered by the writer to be of special interest in connection with the question of homothallism and heterothallism will be emphasized. It should be noted before continuation of this discussion that a much needed revision of terminology and genetic symbols for the various cultural types of *Glomerella* is now in progress and will be published at a later date (personal communication from H. E. Wheeler). Until this publication is forthcoming, it is considered advisable to continue the use of the terminology employed in previous papers.

Four main cultural groups have now been recognized. These are "plus" (AB), "minus" (Ab), Conidial A (aB), and Conidial B (ab). The differences in these cultures have been shown to be brought about by the presence of certain genes at two linked loci.[12] Actually, three allelomorphic factors have been identified for each locus, but these findings are to be published at a later date.[18] The four main cultural groups are readily distinguishable phenotypically on artificial media. "Plus" cultures are light in appearance and produce perithecia in relatively large clusters, while "minus" cultures are dark and have scattered perithecia. Conidia are few or lacking in both on the media used. Conidial A cultures produce large pink masses of conidia, while Conidial B cultures produce conidia in small groups. Neither of these produces any perithecia. Two types of "plus" cultures, Plus A and Plus B, and two types of "minus" cultures, Minus A (the common "minus" culture) and Minus B, have been recognized in publications up to the present time. Plus A, which is what almost inevitably appears when the fungus is isolated

from its natural substrate (various host plants) and may therefore be referred to as the "wild type," is the same as Edgerton's original "plus" culture. The other cultures were obtained from the "wild type" culture either directly or indirectly, as the result of mutations.

A large number of the perithecia of the Plus A culture grown alone give rise to asci in which four ascospores are "plus" and four are "minus," or else all eight are "minus." Apparently, a smaller number of perithecia produce asci with all eight ascospores of the original "plus" type. This is an unusual phenomenon and apparently has not been previously reported in the fungi. Recent studies[14] show that this is brought about largely through the presence of a gene *M*, which influences the mutation of the gene *B* to *b*. Thus AB, through such a mutation, becomes Ab, which is the "minus" genotype. When the mutation has occurred (most likely followed by the division of the mutant nucleus), the fungus appears to exhibit a preference for the "minus" type nucleus in sexual reproduction, so that nuclei of "plus" and "minus" genotypes or nuclei of the "minus" type only frequently pair (and later fuse in the asci); whereas, there is apparently less of a tendency for two "plus" nuclei to pair.

The Plus B culture, which is very similar in appearance to Plus A, differs from the latter in the absence of the gene for mutation. Its perithecia give rise to asci with all eight ascospores of the "plus" type. Mutations to the "minus" genotype are rare. Minus A, which is probably the same as Edgerton's original "minus" culture, produces perithecia which are rarely fertile. The few ascospores which may appear are all of the Minus A type. Minus B produces numerous fertile perithecia, the ascospores of which are all of the Minus B type.

Although the Plus A or "wild type" culture might possibly be considered homothallic, it is obviously not a stable representative of that type of life cycle. Only the Plus B and Minus B cultures may be considered as typically homothallic in their behavior when grown alone. If only the Plus B or the Minus B culture were known, no one would question the homothallic nature of the organism. However, when the different cultures are paired in various combinations, the situation becomes more complicated. Any one of these cultures can be successfully crossed with one or more of the others, resulting in the production of fertile perithecia with hybrid asci. Two self-fertile cultures—that is, either "plus" culture and the Minus B culture—are readily crossed and perithecia with hybrid asci are produced in the line between the two cultures. One of the most interesting crosses involves Minus A, which is almost self-sterile and produces mostly infertile perithecia, and Conidial A, which produces only conidia when grown alone. A line of perithecia with well-developed asci and ascospores forms between the two cultures. These ascospores are of the four genotypes which would be expected: Minus A, Conidial A, Conidial B, and "plus." Even more unusual than this will be the forthcoming report of crosses between two cultures, one a "plus" culture in which ascospore formation is completely suppressed by a newly discovered gene and the other a conidial culture. These crosses result in the production of fertile perithecia in the line between the two cultures (Wheeler and McGahen, to be published).[18] One of the most interesting results of these crosses is the recovery of Plus B and Minus B cultures from the ascospores, these two cultures having a homothallic type of development when grown alone.

It is obvious, therefore, that a condition resembling heterothallism, as well as homothallism, exists in this fungus, when these terms are applied in the broader sense in which they are generally defined. It is equally obvious that neither homothallism nor heterothallism has reached a stabilized condition in the organism. Whether a crossing of strains occurs in nature has not been determined, but the possibility that it

may occur has been demonstrated. It has been shown that the "wild type" or Plus A culture consistently produces large numbers of Minus A ascospores in addition to Plus A ascospores, and it has been demonstrated that Plus A and Minus A may be readily crossed on artificial media. It is quite possible that we are witnessing some of the steps in the evolution of a fungus from a homothallic to a heterothallic condition. If so, it seems likely that such a transition may be brought about by a few genic changes.

In order better to understand how cultures behave when grown alone and when crossed with each other, it is necessary that attention be given to studies on the morphology and cytology of the organism. These investigations show that the perithecium originates (in single cultures and in crosses) from a pair of uninucleate initials which arise as short branches from adjacent cells of a hypha. These two initials were called antheridium and ascogonium by Hüttig,[19] but the recent investigations of McGahen and Wheeler[20] do not support this concept. The latter investigators find that one initial surrounds the other, the inner one becoming the ascogonial coil, the outer developing into the perithecial wall. At an early stage, a copulation hypha grows into the young perithecium and fuses with the tip of the ascogonium, thus initiating the dikaryophase in the ascogonium. In a perithecium-forming culture grown alone, the copulation hypha arises from the same hypha that produces the perithecial initials. In crosses, the copulation hypha is produced by one culture, the perithecial initials by the other. For example, in a cross between a "plus" and a conidial culture, plasmogamy has been observed between a copulation hypha produced by the conidial culture and ascogonia of young perithecia produced by the "plus" culture. Thus, the mechanism of crossing in two *Glomerella* cultures is elucidated.

Croziers proliferate from a single dikaryotic cell of the ascogonial coil.[11] Thus, a single pair of nuclei, through repeated conjugate divisions, produces all the nuclear pairs for all the asci in a single perithecium. This explains why all the asci in a single perithecium are genetically alike. Nuclear fusion occurs in the young ascus, and this is followed later by three successive nuclear divisions, the first two of which are meiotic.[10, 11] Eight haploid ascospores are produced and eight ascospores are cut out in the ascus. The fusion nucleus in the ascus contains four bivalent chromosomes, a set of four being contributed by each of the two nuclei that fuse in the young ascus. The diploid number therefore is eight. The haploid number, found in the ascospore nuclei and in all other nuclei of the fungus, is four. The cytology of ascus development is clearly similar to that reported for most other Ascomycetes. This further confirms the conclusions arrived at through genetic studies that the phenotypic differences and behavioral patterns of the various cultures of *Glomerella* are explicable, not on the basis of differences in chromosome numbers, but rather on the basis of gene differences at a few loci.

Investigations now being carried on by the Louisiana group of workers and by others who have become interested in the *Glomerella* problem should add much to our knowledge concerning the genetic processes which control compatibility and incompatibility in an organism.

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THE TRUFFLE FUNGI*

By DONALD P. ROGERS†

The interest and difficulty belonging to the study of truffles, and their biological and evolutionary relationships, all arise from the fact that they are subterranean. Truffles are the fruiting bodies of a group of fungi, not only formed beneath the soil, but matured there. Subterranean fructifications occur also in the Endogonaceae (highly developed relatives of the black bread-mold), several families of gasteromycetes (like the better-known families, the puff-balls, believed to be composed of highly specialized descendants of the gill fungi), and the Elaphomycetaceae (near kin of *Eurotium*, the perfect stage of many of the green molds). These may be referred to as false truffles. The true truffles, members of the Tuberaceae, differ from the Endogonaceae and the several gasteromycetous families in reproduction by ascospores, rather than zygo- or basidiospores, and from *Elaphomyces* in the production of asci in regular layers to form hymenia or, lacking that, in tender, fleshy texture, large asci and spores, lack of a distinct firm rind, and persistence of asci until maturity.

Since truffles are subterranean, the mechanism of spore-dissemination by explosive discharge of the asci has no value and has been lost. Just as the student of fungi is unable to determine from gross morphology alone whether certain Basidiomycetes should be assigned to the agarics or the gasteromycetes, and must decide the point from the persistence or loss of the mechanism for discharge of the basidiospores, so among the higher Ascomycetes the mycological criterion of a truffle is that the ascus cannot expel the ascospores. Truffles are therefore disseminated not by airborne spores, but, so far as is known, by animals, certainly rodents and carnivores, probably ungulates, possibly insects. The fleshy fructification in which the spores are borne, and the delectable, or at least penetrating, odor characteristic of many species, are, like the pulp of a berry, bait for the animal that will eat the truffle and scatter its spores.

What are generally believed to be the most primitive of the Tuberaceae are cup-shaped, or nearly spherical and with an opening to the exterior. Such fruiting-bodies are lined by a regular layer of club-shaped asci interspersed with paraphyses; spores, texture, and asci closely resemble those of operculate cup-fungi, and, since the latter sometimes occur half-buried, only the absence of an operculum or pre-formed ostiole in the ascus indicates the systematic position of the fungus. The close resemblances of such Tuberaceae to members of the Pezizaceae provide satisfactory evidence that the truffles are degenerate (so far as the spore-discharge mechanism is concerned) or specialized (for animal dissemination) descendants of cup fungi. In more highly evolved Tuberaceae, the hymenium is progressively overlain by intertwined paraphyses or the sterile walls of the fructification; the fruiting areas come to occupy an irregular and contorted series of folds and later of separate chambers, rather than a simple cup; the asci lose the

* This paper, illustrated with lantern slides, was the second of two presented at the meeting of the Division of Mycology on March 23, 1951.

† The New York Botanical Garden, New York, N. Y.

narrow form associated with projection of the spores, as the series evolves, becoming pyriform and in the most greatly modified genera broadly oval; and the hymenial arrangement is lost, so that the asci are finally as irregularly placed as in the lowest Ascomycetes. All of these phyletic modifications are comprehensible as loss of morphological characteristics of no value in subterranean fruit-bodies.

Truffles are associated with the roots of various species of seed plants—so far as known, of woody kinds. The fact that the association is not indiscriminate, but that a species of truffle has a limited number of plant associates, suggests that the relation is symbiotic; and this has been demonstrated by the tracing of mycelium from truffles to the mycorrhiza of trees. Before such a biological attachment was suspected, the knowledge that the most highly valued species were found about "truffle oaks" was the stock-in-trade of those who gathered them for the market.

The high gastronomic and market value given to a number of old-world truffles led to the development of a means of collection more dependable than the human body can provide: in France and Italy, truffles are uncovered by pigs or dogs trained for the purpose. The more efficient means of collection has led, in turn, to a considerable knowledge of European species. Except in the far west, where a small group of persistent mycologists learned much concerning the indigenous species, a good deal less is known of American truffles. While we do have some sixty species, among them species of excellent flavor, and various genera unknown elsewhere, thus far, no one has found in America any of the principal commercial truffles of Europe.

SECTION OF ANTHROPOLOGY

THE CHINESE OF HAWAII: THEIR ROLE IN AMERICAN CULTURE*†

By F. L. K. HSU‡

I

The racial composition of Hawaii's population today is roughly as follows: Caucasian, 32 per cent; Japanese, 32 per cent; Hawaiian and part-Hawaiian, 15 per cent; Filipino, 10 per cent; Chinese, 6 per cent. The remaining 5 per cent is made up of Puerto Ricans, Koreans, and others. These percentages have been substantially and relatively the same during the last twenty years, except that for Caucasian, which has doubled itself during this period. Over 95 per cent of the Chinese are situated on Oahu Island, of which Honolulu is the principal city.

The bulk of the original Chinese in Hawaii came in the latter part of the 19th century, as illiterate, indentured laborers on the plantations. Their initial contract usually provided for a period of three years of work in the island in exchange for free transportation, food and lodging, and a wage of about three dollars a month. Most of them were drawn from one district near Canton. Some ten or fifteen per cent were from four neighboring districts.

Today, most of the Chinese in Hawaii are second, third, and fourth generation Americans. They have spread over the entire economic and social ladder of the territory. In 1882, about fifty per cent of the workers on the sugar plantations were Chinese. In 1934, though the total number of workers on plantations had increased by about four and one-half times that of

* This paper was presented at the meeting of the Section on March 26, 1951.

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1882, only 1.5 per cent of the workers were Chinese. In 1890, about 11 per cent of Chinese males gainfully employed were engaged in "preferred" occupations. In 1930, the corresponding figure was 50 per cent. In 1890, only three per cent of gainfully employed Chinese females were engaged in "preferred" occupations. In 1930, the corresponding figure was 70 per cent.

Residentially, Chinese are found in every census tract of Honolulu. This picture was already true in 1920 and is certainly true today. Even in 1930, only 47 per cent of the population of the so-called "Chinatown" of Honolulu were Chinese. In that year, only 30 per cent of the population in areas adjacent to Chinatown were Chinese.

In school attendance records, the Chinese in Hawaii topped all racial groups. In 1930, 75.8 per cent of Chinese 5 to 24 years of age were in school, as compared with 60.3 per cent of Hawaiians, 57.7 per cent of Caucasians, and 71.5 per cent of Japanese in this age group. In 1940, the Chinese retained their first place in school attendance record. Today, most Chinese in Hawaii speak excellent English and live in American style. They promote boxing and play American football. They are found in elective offices, such as territorial senators and representatives, and in professional ranks, such as doctors and lawyers.

II

By the usual standards, then, the Chinese of Hawaii are very American, except that they are not White. However, if we look closer and deeper, we see patterns of behavior among the Chinese of Hawaii, today, which differ in a number of ways from those which characterize Americans. Most Americans, as well as the majority of Chinese in Hawaii, will object to this statement. It is, however, the purpose of this paper to show that, while it is impossible for any people with a distinct cultural background to erase that background within a matter of three or four generations, especially if they live in a more or less circumscribed area, such as an island, these differing characteristics carried by the Chinese, far from being undesirable, may become, contrary to the usual assumptions, a foundation for some invaluable contributions of the Chinese to American culture and society as a whole.

What are some of the characteristics of the Chinese in Hawaii today? First let us look at religion. Practically all Chinese born and raised in Hawaii in recent years have become Christians. Yet they do not seem to care if they are not all Protestants. In fact, there seem to be as many of them Catholics as Protestants. The activities among Chinese Catholics are certainly as evident as they are among Chinese Protestants. In addition, there are a large number of Chinese Mormons and some Chinese Bahaians.

This poses a very interesting question: Without any Christian background when they came, how did it happen that they took to Catholicism and Mormonism as much as they did to Protestant denominations, especially since Protestantism has the dominant religious value in American culture? Two further facts are to be noted in this connection: 1) It is a rule, rather than an exception, for members of the same family to have different religious affiliations, such as, for example, a Catholic father, a Methodist son, and an Episcopalian daughter, while the mother retains her custom of worshipping at Chinese temples. 2) Differences in religion do not seem to hamper Chinese family solidarity. Frequently, Christian Chinese have told me that if and when their mothers asked them to go to Chinese temples they would do everything the elders requested. They did not see any conflict with their own church activities.

What is observable in religion seems to be evident also in the organizational activities of the Chinese in Hawaii. In the first place, most Chinese belong to organizations traditionally Chinese in nature. These organizations are based upon territorial ties (such as district and village), kinship ties (such as clan), occupational ties, or

recreational needs (such as boxing and music). A relative minority of Chinese belong to modern organizations, such as the women's clubs or university men's or women's clubs, or organizations in support of the Community Chest, a cancer fund and information, or Chinese schools.

In the second place, very few of even the modern organizations are based upon a definite cause. Most of them are mere social clubs. Apart from the cancer and some temporary China relief organizations, there is nothing comparable to the Society for the Prevention of Cruelty to Children, the Society for the Prevention of Cruelty to Animals, the Anti-Vivisection Society, the Conference of Christians and Jews, or the Women's Christian Temperance Union.

The family among the Chinese in Hawaii is equally interesting. All the second, third, and fourth generation Chinese in my experience consider freedom of choice in marriage essential. Teen-agers, as well as older students, use terms like "dating," and do go out with members of the other sex freely. But the courtship lacks many essential American qualities. For one thing, Chinese boys seem to complain that Chinese girls are far too cocky and far too concerned with their own dignity rather than with popularity with the boys. Several boys told me that when they went out with Chinese girls to a dance, their dates often went off and danced with other boys most of the night. Then, Chinese boys agree that one can't "make" a Chinese girl, as one can girls of other races. In general, Chinese men and women in courtship seem to lack the ardor, demonstrativeness, and ecstatic devotion common among young American lovers.

In the marital relationship, the Chinese in Hawaii have something equally distinct. The vast majority of Chinese families, judging by census reports and personal observation, would seem to consist of a husband, wife and unmarried children. But the relationship between the spouses and between parents and children is much less marked by individuation and much more by a pattern of taking each other for granted. There is little question of "wooing your husband or wife anew each day," and parents have little private life from which their children are excluded.

One other thing of interest is connected with names. Practically all Chinese born and raised in Hawaii have non-Chinese first names. However, they do not only have Harry Wongs, or Mary Chongs, but they also have a large number of non-Anglo-Saxon looking names. The extreme examples are Maysevene, for a girl who was born on May the seventh, or Worldster, for a boy whose father was interested in the the unity of mankind.

III

What is the meaning of these characteristics among the Chinese in Hawaii? I think they can be understood best if we introduce a hypothesis which I have elaborated elsewhere.* The hypothesis poses the Chinese as being situation-centered in their personal and cultural orientation and the American as being individual-centered. A situation-centered pattern tends to produce individuals whose main attempt will be to find a satisfactory adjustment with the external environment of men and things. The chief concern of such a psychology will be to fit in or harmonize with what exists. The individual-centered pattern produces individuals whose main attempt will be to find ways and means of fulfilling their own mental requirements. The chief concern of that psychology will be to bend the external world of men and things according to their predilections. The situation-centered personality is more realistic and works toward compromise, but the individual-centered personality has greater emotional intensity and aims at all or none.

In spite of this contrast, however, competitiveness enjoys a high value in both cultures. This quality explains such things as the economic successes of the Chinese in

* Hsu, F. L. K. 1949. Suppression *versus* repression, a limited psychological interpretation of four cultures. Psychiatry. XII (3):223–242.

Hawaii and their lead in school attendance, but it is the contrast between situation-centered and individual-centered orientations in Chinese and American cultures which explains the most distinctive characteristics of the Chinese in Hawaii.

First, religion. In China, in spite of centuries of missionary work, less than one per cent of the population today is Christian. Being members of the American society, in an environment where Christianity has a much higher degree of social importance than in China, the Chinese in Hawaii naturally embrace it. But they have neither the militant Christian spirit characteristic of pioneer Americans, nor any feeling of superiority toward non-Christians. This is perfectly consistent with a situation-centered outlook. This outlook, in contrast to the individual-centered outlook, is inconsistent with a religion in which the major emphasis is on a personal, direct, and introspective relationship with God and in which all other gods are false and therefore must be eliminated.

Given this background, it is easy for the Chinese of Hawaii to embrace not only different sects of the same religion, but even different religions, within one family. The high proportion of Catholics is of especial interest. One of the outstanding private schools in Hawaii is Saint Louis College for Boys. Many Chinese are Catholics because they studied or graduated from this school, which happens to offer excellent training in commercial arts, such as bookkeeping. To a people with a situation-centered orientation, but without an emotional intensity which will make a personal religion of overwhelming importance, the external advantages, such as those offered by Saint Louis College, are likely to be of much importance. In China, too, of the very small number of Christians, 90 per cent are Catholics and 10 per cent are Protestants. In China, also, over the centuries, the only group that created disturbances of a religious nature consisted of the monotheistic Mohammedans. To the Chinese, religion is a means through which an individual will find his place among men or gods. Even if he accepts monotheism, he is far too reasonable to be hostile to other beliefs.

The Chinese way in organization is no less illuminating. In general, two things stand out. The Chinese organizations in Hawaii, whether traditional or otherwise, tend to be inclusive rather than exclusive, in contrast to the usual Americen way.

The simplest expression of the American psychology of exclusiveness is found in the so-called restricted neighborhoods. When a number of well-to-do families moved into one end of a lower income street in a Connecticut town, they renamed their half of the street. The widest expression of this psychology is racial discrimination. This is a fact too well known to need documentation. In fact, one of the most important aspirations of the average American is locked in a paradox. On the one hand, he wants to be one of the fellows. On the other, he takes pride in being a member of an exclusive group, from which others are excluded.

On the contrary, even the new organizations of the Chinese in Hawaii fail in this characteristic. Even the now-famous American-Chinese Club tends to be more interested in its business possibilities (such as having its hall rented for entertainment purposes, at a profit, and declaring a dividend among its members toward the end of the fiscal year), rather than in making the club inaccessible to aspirants. I have often been told by members of this club that it was started in reaction to some of the restrictive attitudes on the part of some of the Haole* clubs, yet the American-Chinese Club has no scruples about admitting anyone who is part-Chinese.

The second thing to be observed about Chinese organizations in Hawaii is that they have little of the crusading spirit. This is so even with the two so-called secret societies, which allegedly began as an attempt to overthrow the Manchu Dynasty

* Hawaiian term for Caucasians, excluding Portuguese.

in China. It is to be recalled that in the history of the WCTU movement in the United States there were a number of outstanding women who individually went out of their way to smash up saloons. This spirit is utterly alien to Chinese organizations. There are some Chinese organizations to help cancer research or the Community Chest, but these are minor. One may argue that the Chinese in Hawaii do not need a WCTU movement because alcoholism is not a problem among them, but as will be evident below, there certainly is a need for something which might be called the "Chinese Society for the Prevention of Gambling." No such organization exists, however. In other words, there are, in the normal course of events, hardly any Chinese organizations, whether in Hawaii or China itself, whose aim is to do something for people, to reform or improve them, whether they want to be reformed or improved or not, or to annihilate other people or groups.

Lastly, let us look at the family. Given the opportunity, Chinese youngsters like to choose their own mates and set up their independent households, and name themselves and their children in accordance with the fashions of the times and place, but they are not likely to insist upon these things regardless of consequences. They do not have the one-mindedness which is fostered in the American culture so that they will insist on all or none. They are much more prepared to accept a compromise situation so that in a matter of choice of a mate they will consider the wishes of their parents almost as much as they will be motivated by their own desires. In their sexual love for each other, they will be concerned over what other people say as much as they are conditioned by their own preferences; and in their marital adjustment they will safeguard their individual happiness as much as they will be affected by the permanence of their marital ties once the wedding bells have been rung. For these reasons, the Chinese in Hawaii are not yet afflicted by two of the most vexatious American problems today. (1) The Chinese in Hawaii have no problem of the aged. This is so, not because the Chinese support their elders more, but because they are not given to the idea that old people are a stumbling block to the happiness of the young. According to the Chinese situation-centered culture pattern, the old have merely advanced to another stage of life in which they have a new significance. Being old is not synonymous with becoming useless or being relegated to the background. Therefore, the old can age gracefully and with dignity. They do not have to "refuse" to get old, nor do they have to hold onto the young for fear that once they let go they will have no respected place in the scheme of things. (2) The Chinese in Hawaii are not troubled by adolescent difficulties. Both police files and interview material with social workers and Chinese parents show that the problemed adolescent is an exception rather than the rule. This is at least partly related to the lack of old-age problems. Since the old do not have to hold on to their young with great emotional intensity, there is little reason for the young to react to the old strongly. Since children and parents maintain little privacy of activities, the adolescent is far too realistic to want to abandon all his social ties because of some momentary pleasure or hatred.

IV

It has often been argued that Orientals are masters of self-control and therefore, inscrutable. This is sheer popular misconception. No people can control their emotions permanently and in every direction. If the Chinese in Hawaii have merely engaged in more self-control than the majority of Americans, then we should also have some indication of this fact in their inevitable breakdowns, such as crime, suicide and mental illness; but the facts are quite contrary to such an assumption.

For purposes of this paper, three kinds of crime are distinguished. They are: (1) rape, assault with intent to rape;

assault with intent to ravish, and attempt to commit rape,—all sex crimes*; (2) manslaughter, robbery, burglary, larceny of all kinds, auto theft, forgery and embezzlement; and (3) gambling. Honolulu police reports for random years (1917–18, and 1925 to 1940 inclusive) and records from all courts of Hawaii for sixteen years were examined. It was found that the number of Chinese in these reports, compared with Whites,† is not only disproportionately smaller in the sex crime charges and convictions, but also disproportionately under the manslaughter, *etc.*, charges and convictions. Under the category of gambling, however, the positions between Chinese and Whites are reversed. Here the Chinese figure is disproportionately above that of the Whites.

The connection between sex crimes and runaway emotions is obvious. Even manslaughter and burglary, *etc.*, would also seem to require much overpowering of reason by emotion. On the other hand, gambling is undoubtedly the least emotional of all the ways of getting into trouble with the law.

Next, material on suicides and attempted suicides was examined for the years 1936 to 1950 inclusive. The overall suicide rates for Chinese for the fifteen years are slightly higher in proportion to the population than that for the Whites. But if suicide and attempted suicide cases are combined, the figure for the Chinese, whose population is little over one-fifth more the Caucasian, comes to less than one-sixth of the corresponding figure for Whites.

For the Chinese, suicide is a very serious business and they must have good reasons for taking that road. Being of a calm nature, they can make sure of succeeding; but being situation-centered, they do not easily take the road of desperation, not only because of their greater consideration for the people that surround them, such as family, relatives, neighbors, and friends, but also because they do not have emotions which easily disturb them. The Whites, on the other hand, are more pressed by emotion and less hampered by situational considerations. For them, success is a greater individual triumph, but failure is correspondingly a greater personal misery. They are, therefore, likely to feel desperate much more frequently than the Chinese and they take rash actions. Being more disturbed emotionally, they are often unable to be sure in their methods of suicide.*

These contrasts stand out even more strongly when sex is considered as a factor. The ratio of suicides to attempted suicides among Chinese males is about 1 to 1. Among White males, the ratio of suicides to attempted suicides is 1 to 2.3. Females of all racial groups seem more often to attempt suicide than actually accomplish suicide; but while the ratio of suicide to attempted suicide among the Chinese females is 1 to 4, the corresponding ratio among the White females is 1 to 11.6.

What has been said about crime and suicide is corroborated by some data on the racial incidence of known cases of mental illness in Hawaii. The difficulties involved in the classification of mental illness are obvious, but when two different sources agree, and when both sources are again in agreement with other aspects of life, we have reasonable grounds for taking them with some scientific seriousness. In one study, an analysis of the admissions figures for the year 1947 to the only receiving hospital for psychiatric disorders in Hawaii shows the incidence of mental illness per 100,000 population to be highest among the Whites and lowest among the Chinese.† The rate of organic psychoses, however, remained constant in all groups.

These results are matched by figures on "new registrations by race" supplied by the Bureau of Mental Hygiene of the Board of Health of Hawaii, for the years 1947,

* Usually, in police and court files, the term "sex crime" also includes such other things as adultery, seduction, *etc.* In this paper, the term refers to the above-named categories only.

† The term "White" here includes the "Portuguese" and "other Caucasians" in this and other reports.

* In the corresponding figures pertaining to the Japanese in Hawaii, the contrast to Whites is even more pronounced. This will be dealt with elsewhere.

† Wedge, B. & S. Abe. 1949. Racial incidence of mental disease in Hawaii. Hawaii Medical Journal: 337–338.

1948, and 1949. Here the rates for Whites, though not the highest of all, were double that for Chinese, which again was the lowest.

So whether we look at the normal activities, such as religion or organization, or at abnormal situations, such as mental illnesses or crime, the data are consistent with one another, and with the patterns of competitiveness and situation-centered orientation of life.

This analysis leaves something to be desired, however. It fails to explain why the Chinese will not, at least outwardly, take on all ways of life according to the prestige standards in American society. For example, the highest prestige in America accrues to the White Protestant. If the Chinese in Hawaii are competitive and oriented toward a satisfactory place in the external situation, they should at least want to be Protestant even if they cannot be White.

Again, being an American means to speak good English, to exhibit American manners, and generally to belong to the proper age and status groups. Two investigators in New York City have independently informed me that they have great difficulty in getting college or high school students who have European-born parents to tell them something about their homes or to introduce them to their elders because they are ashamed of the old folks and their ways of life. If the Chinese in Hawaii are competitive and oriented toward a satisfactory place in the external situation, they should at least reject their elders or attempt to do so, like the children of some New York immigrants, especially in view of their own physical differences from White Americans.

These seeming paradoxes will become comprehensible if we introduce a new factor. This is the attraction of the Chinese family and, through it, certain phases of Chinese culture, to the individual. The Chinese kinship organization offers the individual a primary sense of belonging, permanence, and security. It will take a great pull elsewhere to make the individual forego the advantages of being a member of such an organization. That is partly why there is not only little inducement for breaking with the older generation, but there is little crime committed against members of the family.

The advantages are, of course, not without their darker side. Because of the desire to harmonize with the external world and a lack of incentive to break with the older generation, the younger Chinese in Hawaii have little tendency to leave the beaten paths, to be adventurers, to explore new ground and to blaze a new trail, which qualities have, to date, been the mainstay of America's greatness.

V

Having placed some cultural characteristics of the Chinese in Hawaii in perspective, we must now ask two questions. Will the Chinese in Hawaii, in the generations to come, lose their present characteristics? If that should be the case, will it be the most desirable thing for American culture as a whole?

Our answer to the first question is simple. All behavior characteristics which distinguish one people from another are traceable, as far as the science of man has revealed, not to blood, but to cultural history and social organization. As the latter change, the behavior characteristics will change rapidly or more slowly. If a Chinese child is raised entirely in a White home, a White neighborhood, a school of White children, and a society of White individuals, there is little likelihood that he will develop any of the Chinese characteristics portrayed above. On the other hand, as the Chinese in Hawaii live today, their departure from their cultural background is likely to be slower. But with each new generation, the original Chinese characteristics will increasingly diminish.

This answer leads us to the second question, the answer to which is more complicated. When the term "Americanization" is used, consciously or unconsciously, it

refers to two things: One, the process whereby the individual acquires the Anglo-Saxon characteristics of American culture, speaking English, being like Whites in racial characteristics, and embracing Protestantism in religion; two, an emphatic hatred toward all things foreign to the above. For example, a recent refugee from Austria proudly told people that he would not eat anything that he could not order in English and that, as a result, he got along for weeks on doughnuts and coffee. This man might have a personal reason to exaggerate his contempt for his foreign heritage, but his militant attitude toward things foreign is by no means unusual in America. Americans, generally, acknowledge themselves to be bad linguists. But it is not often understood that the root of this apparent lack of ability in foreign languages is to be found in the life in American schools. School children of any foreign background do not dare, for fear of ridicule, speak any other language than English. All enlightened Americans are troubled by racial difficulties in the United States, but it is not often pointed out that the usual attitude toward Americanization is at the root of this intolerance. Since culture, no less than the mind, has an essential unity of its own, it is hardly possible for Americans to be racially tolerant as long as being an American is such a militant fact and as long as all foreign peoples and things are explicitly or implicitly regarded as inferior.

From this point of view, it will be a great loss to American culture if the coming generations of Chinese of Hawaii should lose all their distinctive characteristics; for these characteristics may possibly serve as a moderating factor within the context of American civilization. This is not to minimize the significance of American cultural heritage so far. Without the spirit embodied in the creed of the Protestant pioneers of America, American civilization could hardly have risen to its present height and the American people, as a whole could hardly have achieved their colossal material prosperity. Chinese cultural characteristics, with their emphasis on harmony with the external world and lack of crusading zeal, could not have done these.

However, what America needs today is not only more physical and industrial conquests, but also more consolidation of the physical gains in human relationships, not only within the United States, but also with the rest of the world. For this purpose, an individual-centered orientation, with its high emotionality and emphasis on all or none, will not fill the bill. If we can understand and accept this fact, and can make some of the qualities of the Chinese in Hawaii truly and actively a part of the great American heritage, we may yet achieve a greater America, as the nation which will offer to mankind not only the technical know-how, but a social architecture which will form a solid foundation for the peace and prosperity among mankind as a whole.

VI

Having concluded that there is a need for a synthesis of some kind between the cultural characteristics of the Chinese in Hawaii and those of America is one thing. How to bring about this synthesis, in face of the present-day tense social climate of America, because of international developments, is quite another. We may have to modify our views on Christianity to achieve this synthesis. We may have to change some of our attitudes toward Americanism. We may have to tell our children that Americans do not necessarily have the best of everything in the whole wide world. Finally, we may have to admit that there are great men in every land, just as there are George Washingtons and Abraham Lincolns in America. These will be difficult things to do and to speak up for; but as long as we can discuss the question freely, we have hope.

SECTION OF OCEANOGRAPHY AND METEOROLOGY

PROBLEMS IN AIRLINE METEOROLOGY*

By PETER E. KRAGHT†

Synoptic meteorology applied to aircraft operation is beset with all the difficulties that are inherent in applying meteorology to everyday activities of men, as well as with difficulties of its own. It is never easy on the part of everyone to find satisfaction with a forecast. Some are unhappy because the rain came too early and others are happy with the same event. If the forecast is clear and warm weather, the man with a newly-seeded lawn blames the meteorologist for ordering weather that can ruin his grass, but the builder with a dozen houses under construction might be inclined to thank the forecaster for apparently setting the elements in order for his work. The diversity of interests which occupy people precludes satisfaction on the part of all with either the forecast or the actual weather.

Individual dissatisfaction on the part of the public, however, is one of the minor problems facing the meteorologist. Uncertainty in controlling predictors is by far a greater difficulty. To explain controlling predictors, an analogy from physics will be used. Imagine a rock lodged in a small depression on a hillside. The rock is in an unstable position generally speaking, but enjoys pseudo-stability for the moment. It is within the power of a person standing in the hillside depression to push the rock to the edge of the hole and send it rolling downhill with ever-increasing speed. The control in this case is vested in the actions of the person pushing the rock. Take a case from chemistry. An indicator is placed in a basic solution which will change color the instant a very slight preponderance of acid occurs. Drop by drop the acid is added and nothing happens until, at the critical turning point, a fraction of a drop of acid causes the solution to change color instantly. The control parameter here is the amount of acid, which becomes very sensitive at one point in the process, to the extent that a very slight change in the control parameter causes a macroscopic change in the appearance of the whole system.

The critical point temperature in a three-phase system of ice, water, and water vapor is another of these sensitive controlling parameters in nature. A small change in temperature at the critical point causes vast changes in the system. By comparison, large changes in temperature, at some temperature far removed from the critical point, cause no significant changes in the system.

A weather forecaster is faced with uncertainties of a similar nature. Actually, he has control over nothing at all. It is not in his power to push the rock over the depression rim, or to add that last fractional drop of acid to a basic solution, or to raise or lower the temperature slightly at the triple point of water.

Parameters of an atmospheric system must be used as predictors for a future state of the system. From among the many parameters or properties of the atmospheric system, the forecast must select some as predictors for one or more future states of the system. The elements of the future states are called predictands.

To illustrate, consider an atmospheric system consisting of a cyclonic eddy of 500 miles radius with its center over Boston, Massachusetts. The area of interest is that strip of coast from Washington, D. C., to New York City. In this region, the location of the eddy center dictates that the flow shall be from a northwesterly quadrant. This is one property or parameter. The vertical thermal and water vapor distribution are two others. Geographically, the Appalachians cause a slight downhill component in the air flow. Meteorologists recognize this case as one

* This paper was presented at the meeting of the Section on March 27, 1951.

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of traditionally fair weather. The two controlling predictors are: (a) the air flow from the northwest, and (b) the geographical fact that the flow is slightly downhill. All other predictors have little bearing in determining the state of the atmosphere in the New York City to Washington, D. C. coastal strip. This is a simple case in which the controlling predictors are well known, few in number, and dictatorial in their control.

The scene can be reversed. An anticyclonic eddy is located over Maine causing onshore flow in the New York City to Washington, D. C. coastal area. Skies are still clear but the air is approaching saturation at an indeterminate level. No one knows how near the air is to saturation at the critical level, nor has he any manner or method of discovering where the critical level is located. Controlling predictors are the nearness to saturation, the height of maximum degree of saturation, and the rate of eddy transport of water vapor to the critical level, which in turn depends in part on wind and thermal structure. It is the forecaster's job to pick out the controlling predictors from the half-dozen predictors. He must determine the proximity to the critical point of saturation at a given time and the rate of approach to the critical saturation time. Obviously, these are requirements that cannot be met except within limits of uncertainty in the predictors. For this reason, he cannot truthfully state that an overcast will form at a specific hour or minute, but must be guided by the uncertainty factor in the controlling predictors to temper his forecast with a "plus or minus" number of hours.

The public, however, has little sympathy for the "plus or minus" modified forecast. To them, it appears to be uncertainty or hesitancy on the part of the forecaster. That is the heart of the problem for the meteorologist. The honest meteorologist cannot give the public what it demands every day because the meteorologist is seriously limited in the tools he has to work with in too many of the cases.

When the controlling predictors are well established and the atmosphere is far removed from any critical temperatures or other critical points, then forecasters can afford to be audacious, specific, definite. And they usually are. Yet, by so doing, they create a demand for similar forecasts on those days when the controlling predictors are poorly established and there are one or more critical points to deal with in issuing a forecast. On these days of indeterminate parameters, it cannot be so nicely done.

Not all the uncertainty, however, is in the controlling predictors and in the presence of critical points. Space, meteorologically speaking, is something entirely different from space as the man on the street visualizes it to be. Only a limited number of people (a very limited number) have any concept of geography or geographical space. The average person travels to and from home by much the same route daily and has detailed knowledge of a few tens of miles of the earth's surface. Four hundred miles to the northwest are only "words" to him.

On the other hand, atmospheric phenomena have dimensions of the order of hundreds of miles, and travel, in many cases, half way around the globe before disappearing. The boundary of an atmospheric eddy whose dimensions are of the order of 10^3 miles cannot be determined nearer than, perhaps, plus or minus 10^2. For this reason, one hundred miles may not mean very much, meteorologically speaking. Yet this same one hundred miles transcends the whole knowledge of space on the earth's surface held by some little-travelled persons.

Uncertainty in fixing the boundaries of atmospheric phenomena within rather broad limits, as measured by standards of ordinary people, can easily lead to differences of opinion as to whether a forecast was accurate or inaccurate. To the forecaster, if a forecast event happened within fifty or

one hundred miles, that is verification. To the man in the street who has no knowledge of what happens fifty miles away, the forecast was inaccurate.

Time and velocity both contain some uncertainty. As in the concept of space, time and velocity have different meanings to the meteorologist than they do to the man in the street. It is common experience for a forecaster to regard the speed of atmospheric phenomena as determinable only between certain limits. For example, the eastward speed of a cyclonic eddy and its accompanying rain shield is known to be 30 miles per hour plus or minus 5 miles per hour. In other words, the velocity of the system is determinable only down to a range of values between 25 and 35 miles per hour. In ten hours, using 25 miles per hour, the edge of the rain will have moved 250 miles. Using 35 miles per hour, it will have moved 350 miles. Thus, in ten hours, an uncertainty of 100 miles in forecasting the leading edge of a cyclonically induced rain shield accrues from an uncertainty in speed of the system of only plus or minus five miles per hour. In 20 hours, the uncertainty grows to 200 miles and, in one day, to 240 miles. If a plus or minus factor is not permitted in the forecast, the forecaster has of necessity to select the displacement based on an average velocity, knowing that nearly all cases will vary from the average by the plus or minus factor.

To the recipient of the forecast, six o'clock means just that. That rain did not begin at six o'clock means an inaccurate forecast to him. To the forecaster, onset of rain anytime between two o'clock and ten o'clock is a verifying forecast.

Sometimes the problems faced by the forecaster serving the public are of lesser magnitude than those of the forecaster serving aircraft-operating interests. This is the result of dealing with operating people who have a comprehensive knowledge of time and space, both geographically and navigationally. Some of the problems carry over into the aircraft-operating field and new ones are added. The uncertainty in dealing with the controlling predictors under some circumstances and the proximity of a critical state both carry over into the operating field. The crucial necessity for safety introduces other problems, namely the necessity of biasing a forecast on the pessimistic side.

The forecaster serving aircraft-operating interests is no better or no worse off than the one serving the public, in respect to the problem of determining the controlling predictors for future states of the atmosphere. He is primarily interested in factors which influence aircraft operations, namely, ceilings, visibilities, form of precipitation, icing, hail, turbulence, and electrical phenomena. Some of these problems do not come within the realm of interest of the public meteorologist. Each of the phenomena, however, has its own all-too-often indeterminate controlling predictors. Cumulonimbus phenomena of hail, severe turbulence, and static charge and discharge have no certain predictors because they change from moment to moment and location to location inside the cloud. One is restricted to forecasting the occurrence of cumulonimbus without specifying when and where the violence accompanying the cloud will occur.

The same is true of icing. The size of the water droplets and their depression below zero centigrade are the controlling predictors for icing, but neither one is certain within very broad limits. Furthermore, icing is known to change from moment to moment and place to place to the extent that two aircraft a few minutes apart at the same altitude over the same area have widely differing icing experiences.

The form of precipitation usually has a drastic influence on operating procedures. Rain is of little consequence except in reducing visibility and forming low scud clouds. Freezing rain stops or seriously hinders all operations. Heavy wet snow is as much of a hindrance as freezing rain. Heavy or even moderate snow usually reduces visibility to a distance which cur-

tails or ends flying. In many winter cyclonic storms, the form of the precipitation remains indeterminate until one definite form is established. The indeterminate nature arises from two uncertain factors: (a) the presence of a critical temperature at a critical altitude, and (b) uncertainty in the controlling predictors. Statistically, a freezing level at roughly 3000 feet above the ground more or less guarantees rain and, in this respect, is a positive predictor once it is established as being at 3000 feet or higher. If the freezing level is below 2900 feet, the form of the precipitation may be snow or rain or both, and there is no really certain controlling predictor. Nearly always the forecaster has only vague and uncertain knowledge of both the proximity to the critical state and of the controlling predictors.

The necessity of insuring safety in all aircraft operations dictates a bias in operating forecasts toward the pessimistic side. Suppose there were one hundred ways of arriving at a forecast for a ceiling at a given time. These forecasts arranged graphically about the mean or average value would produce a graph approximately equivalent to the standard errors graph, *i.e.*, a function of e^{-x^2}. About half of the forecasts would lie to the optimistic side of the mean value, and the other half would lie to the pessimistic side. A standard deviation can be calculated by regular statistical methods. For purposes of illustration, assume the mean value of the forecasts was 1500 feet ceiling and the standard deviation was 300 feet. Statistically, about 68 per cent of the cases will lie between plus or minus the standard deviation, *i.e.*, 68 per cent of the cases will fall in the range of 1200 to 1800 feet, while 95 per cent of the cases will lie between plus or minus twice the standard deviation, *i.e.*, between 900 and 2100 feet.

A forecast calling for 900 feet, on a statistical basis, would stand a probability of 95 per cent of being equal to or less than, the actual ceiling. In other words, the ceiling stands a 95 per cent probability of being equal to or better than the forecast and only a 5 per cent probability of being less than the forecast. This is the bias which must be given to operating forecasts to assure a safety factor.

An additional safety factor is built into operating procedures by the use of alternate airports. If the probability of a destination airport's being below landing limits is 10 per cent and the probability of an alternate airport's being below landing limits is 10 per cent, the probability of both being below limits is only 1 per cent, provided the atmospheric factors are not the same at the two airports. Adding a third airport with a 10 per cent factor reduces the probability of all three being below limits to 0.1 per cent, a negligibly small figure. Selection of alternate airports should be done as soon as there is any uncertainty in controlling predictors or as soon as there is an approach to a critical state in the atmosphere involving a destination airport.

In summarizing, both critical states in the atmosphere and uncertainty in controlling predictors preclude forecasters from always making positive, definite statements about the future state of the atmosphere. If positive and definite statements are made under uncertain conditions, such forecasts cannot possibly carry a full degree of assurance. The uncertainty problem is partially solved by biasing a forecast for aircraft operations as well as by use of alternates whose local weather will be a function of differing atmospheric states from those at the destination.

Fortunately, there are more days in which there are no critical states in the atmosphere and in which controlling parameters are quite definite. Forecasts issued then can be given a virtually full confidence factor.

NEW MEMBERS

Elected March 22, 1951

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